Popular Astronomy

Vol. XLVI, No. 4 APRIL, 1938 Whole No. 454

On the Variable Magnitude of Gamma Cassiopeiae

By ERNEST CHERRINGTON, Jr.

An effort has been made to work out the light curve of Gamma Cassiopeiae with the thought that fluctuations in the luminosity of the star may be related to the continuous and frequently irregular changes in the intensity of many bright lines in the star's visual spectrum. For this purpose all available observations, totalling 312, have been gathered from the periodicals received at the date of writing. Published observations have been taken from the *Astronomische Nachrichten, Beobachtungs-Zirkular der Astronomischen Nachrichten, L'Astronomie, Bulletin of the Harvard College Observatory,* and *Harvard College Observatory Announcement Cards.* The contributing observers, together with the number of measures published by each, are listed below.

L. Andrenko	50	Poznán staff (2 photometric)	5
R. Rigollet	29	E. Loreta	5
K. Graff (Photometric)	23	C. M. Huffer (Photoelectric)	2
P. Baize	21	J. J. Nassau (Photographic)	2
W. A. Calder (Photoelectric)	20	Mme. Flammarion	2
G. R. Miczaika	17	L. Peltier	2
N. Richter (14 photoelectric)	15	L. Campbell	1
G. Oriano	14	J. Jacchia	1
C. Fedtke	6	Cherrington (Unpublished)	97

The scatter among individual estimates and measures was often large —amounting to as much as 0.6 of a magnitude on a single night in the early part of October, 1936. Mr. Campbell of the A.A.V.S.O. has intimated that such discrepancies represent largely the errors of visual observation due to differential extinction and the absence of suitable comparison stars.[1] He cites the twenty photoelectric observations secured by Calder[2] during the last quarter of 1936 and the early part of January, 1937 (which show variations of less than 0.1 of a magnitude) as evidence of the unreliability of visual estimates. He concludes from this series of measures, some of which were separated by intervals as great as thirteen days, that Gamma Cassiopeiae underwent no appreciable change in luminosity. It is interesting to note that, during the period covered by Calder's observations, the mean light curve based on the estimates of nine observers shows no net change. Increase in luminosity

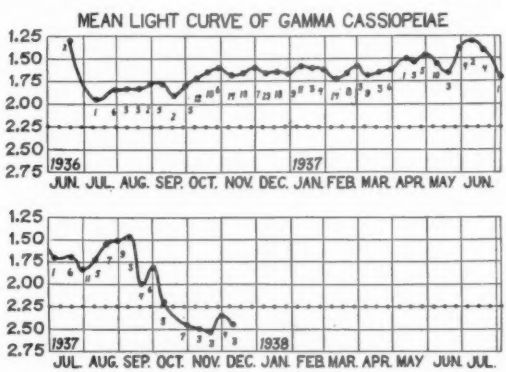
ceased at about the time his series began and was resumed about a month after his series ended. Another interesting feature of the observations covering the last quarter of 1936 is the fact that the greatest scatter among all available individual observations on the nights that Calder used amounted to only 0.2 of a magnitude. Moreover, a difference of fully half this amount exists between the photometric measures of Calder at Harvard and Richter at Sonneberg,[3] both of whom employed photoelectric photometers and both of whom used Beta Cassiopeiae as reference star. Thus it would appear that the photoelectric observations actually confirm our suspicions of short-period fluctuations in the luminosity of Gamma Cassiopeiae. On several occasions, visual estimates made at the Perkins Observatory have indicated changes of several tenths of a magnitude during a single evening. In August, 1936, as will be seen from the curve, the mean of eight observations showed an increase over the normal brightness of about 0.4 of a magnitude while several unpublished photographic observations by Marshall[4] during that month showed no increase whatever. In several instances the published measures of other observers show similar changes. As an illustration, we call attention to two photometric observations recently published by Graff.[5] The measures were made on the night of October 27, 1937, some five hours apart. In each case six comparison stars were used. The mean magnitudes found were 2.28 and 2.40, respectively. It is unfortunate that few attempts have been made to follow the star photometrically throughout the night. Consequently, the data available at present are inadequate for a discussion of short-period variations. Hence, we will consider here only the general changes in luminosity that have taken place during the last eighteen months.

The 312 observations have been averaged to form fifty mean places for the tenth, twentieth, and thirtieth of each month. These have been plotted to give the mean light curve. Below each point on the curve is shown the number of individual observations combined to give that mean place. These numbers range from one to twenty-five. The formerly-accepted magnitude of 2.25 is indicated for reference in the figure by small open circles.

The first striking feature of the light curve is the high luminosity of June 21, 1936, found by Nassau[6] on two pre-discovery plates. The two plates, taken fifty-three minutes apart, gave photographic magnitudes of 1.24 and 1.35, respectively. Although Gamma Cassiopeiae is a B0 star, its position in the Milky Way gives it a color index of zero[7] as the mean of a large number of index measures before the present season of activity. The spectral type, as measured by Graff[8] on June 29, 1936, was A6. During 1937 Graff published eleven determinations of spectral type[9] which range from F1 to F9 somewhat irregularly. Consequently, if any correction should be applied to Nassau's observations to reduce them to the visual scale, the initial point on the curve would doubtless be raised even higher. After an abrupt drop from this point, we see that the

luminosity rose gradually, with minor fluctuations, to a maximum of 1.3 in June, 1937. The rate of rise during July, August, September, and October of 1936 was small. During November, December, and January the curve exhibited the flat appearance already noted. Beginning in February, 1937, the luminosity rose again with a rate slightly greater than the initial rate. During the past summer the fluctuations in luminosity became more pronounced. A striking drop in June and July was followed by a steep rise during August to a second maximum of about 1.5 early in September. The decline in luminosity was relatively swift thereafter. Early in October the luminosity fell below the value adopted previous to the outburst. Since that time the star has appeared abnormally faint on every night we have had an opportunity to observe it.

After the above study had been completed, the light curve as determined by A.A.V.S.O. observers was published by Mr. Campbell.[10] Since the two curves are based on different observations, and since they are not in exact agreement, it seems worth while to publish the ten-day



mean curve. Furthermore, the latter covers a longer period of time than that embraced by the A.A.V.S.O. curve. In comparing the two we shall refer to the A.A.V.S.O. curve as *I* and to the above curve as *II*. *I* shows a constant mean magnitude of about 1.8 from the first of October to the middle of December, 1936, whereas *II* exhibits a continuous rise during October. An increase of about 0.2 of a magnitude in the middle of December shown on *I* does not appear on *II*. Whereas the increase in brightness is resumed in February, 1937, on *II*, *I* continues approximately flat until the first of April. *I* reaches a value of 1.5 during the second half of May and shows a general decline thereafter while *II* attains a value of 1.5 a month earlier, and, after a short drop, soars upward to the maximum value of 1.3 in June, 1937. During August, *II* shows a steady and significant increase while *I* runs approximately constant. The secondary maximum of September is much more conspicuous on *II* than on *I*. Both curves reveal a hump on the first of October,

that of *II* being one-quarter of a magnitude higher than that of *I*. The decrease in luminosity during October is much more striking on *II* than on *I*.

Further evidence of short-period fluctuations follows from comparison of a statement by Fedtke of Königsberg[11] with either of the two curves. Fedtke finds a maximum magnitude of 1.3 in April, 1937, at which time he describes Gamma Cassiopeiae as brighter than Deneb. Thereafter he notes a steady decline, month by month, to magnitude 2.2 on October 13. Fedtke also finds the star approximately constant at magnitude 2.3 at the beginning of December.[12]

The only recent information that has come to our attention on the photometric behavior of Gamma Cassiopeiae previous to its outburst is due to R. Rigollet of Paris.[13] Evidently Rigollet was in the habit of observing this star frequently. He found the adopted value of 2.25 for its magnitude in July, 1928, and again in July, 1930. In October, 1930, he reported a magnitude of 2.20. On twenty-two subsequent occasions (the last of which was July 27, 1935) he recorded this same value as 2.20. On July 13, 1936, he noted a magnitude of 1.95. This last observation, which constitutes the second point on the above curve, should be regarded as the discovery observation although it was not published until after attention had been directed to the outburst of Gamma Cassiopeiae by the announcements of other observers. Rigollet's observations are most interesting since they indicate a slight increase in luminosity six years before the phenomenal rise of the star made it the subject of wide-spread interest.

But now this peculiar star has declined in luminosity to a value which is less than that which it originally exhibited. The fact that Gamma Cassiopeiae is now fainter than its original magnitude is fully as interesting and important as its unusual brilliance during the preceding sixteen months, and it is to be hoped that measurements of its magnitude will be continued. At the present time visual estimates should be entirely reliable since Gamma may be compared with Beta and Delta Cassiopeiae.

There must be thousands of magnitude estimates of Gamma Cassiopeiae hidden away in the notebooks of astronomers both professional and amateur. These would be of great value to us in our endeavor to improve the accuracy of the light curve. Consequently, the writer would greatly appreciate the receipt of any unpublished observations that any reader or his friends may have made or may make in the months to come.

References

[1] Popular Astronomy, **45**, 151 (1937).
[2] *Bulletin of Harvard College Observatory*, No. 905, 29 (1937).
[3] *Astronomische Nachrichten*, **262**, 391 (1937).
[4] *Harvard Announcement Card*, No. 394 (1936).
[5] *Astronomische Nachrichten*, **264**, 163 (1937).
[6] *Harvard Announcement Card*, No. 396 (1936).
[7] See B. W. Kukarkin, *Publications Sternberg State Astronomical Institute*, **10**, pt. 2, 39 (1937).
[8] *Astronomische Nachrichten*, **263**, 381 (1937).

[9] *Ibid.*, and *Astronomische Nachrichten*, **264**, 163 (1937).
[10] POPULAR ASTRONOMY, **45**, 569 (1937).
[11] *Beobachtungs-Zirkular der Astronomischen Nachriehten*, **19**, 66 (1937).
[12] *Beobachtungs-Zirkular der Astronomischen Nachrichten*, **19**, 80 (1937).
[13] *L'Astronomie*, **50**, 572 (1936).

PERKINS OBSERVATORY, DELAWARE, OHIO, DECEMBER 28, 1937.

Shadow Bands

By RICHARD L. FELDMAN

PART I

The subject of shadow bands is one about which little has been published. This is because little has been known about them. In spite of this fact, or because of it, there has been a tendency to ignore them, or at least to belittle them as a phenomenon for investigation.

My contribution is three-fold: (1) I will submit a quantity of shadow band observations, half of them previously unpublished, detailing principally the directions of motion; (2) I will seek to show that the data indicate that the bands are concentric with the moon's shadow, extend to great heights, and move either towards the shadow or away from it; and (3) (in Part II) I will undertake to explain this behavior on the basis of diffraction. Possible interesting consequences from my conclusions suggest themselves, but these must wait upon further verification of my findings.

What are shadow bands? Exhibit A among the observations is the following letter from a Nobel prize winner, whose keenness can make a further contribution to science, though he is now deceased.

WOLCOTT GIBBS MEMORIAL LABORATORY
HARVARD UNIVERSITY
THEODORE WILLIAM RICHARDS, *Director*

Cambridge, Massachusetts, U.S.A., January 26, 1925.

Professor Harlow Shapley,
Harvard College Observatory,
Cambridge, Massachusetts.

Dear Shapley:

As I told you, I went to Newport in order to see the eclipse, and we saw it there most famously. Not having any instruments besides stop-watches and a kodak, my observations are not of fundamental astronomical importance. The eclipse seemed to last 90 seconds ± 1 second by standardized stop-watches; and according to my watch, began at 9 hours, 15 minutes, 30 seconds ± 5 seconds, Eastern Standard Time. These observations are, of course, too inexact to be of any astronomical use.

The only interesting thing which we observed which I have not seen reported in the papers, but which doubtless has been observed more satisfactorily by others, concerned the shadow bands. These were very marked

indeed. They began a few minutes before the eclipse coming somewhat from the north of east; perhaps east-northeast. The bands were about 18 inches apart; according to some observers these were subdivided into much finer bands; but this we did not notice. There was no question about maxima with a distance apart about what I have given. Before the eclipse these shadows were almost vertical on a high board fence behind us and reached off in perfectly straight lines almost in the direction of the sun. It appeared then that there were vertical planes of darkness (if I may so qualify them) sweeping in a more or less southwesterly direction at right angles to the line drawn to the sun. On the white snow-covered field in front of us, the parallel lines pointing to the sun moved from left to right at a speed which has been estimated at 8 or 10 miles per hour. At first they certainly were not faster than this. The lines, converging on account of perspective, gave the effect of a vast wheel with fine spokes moving before us, the spokes pointing toward the sun. It was a very interesting and remarkable sight.

After the eclipse the shadow bands came from the northwest moving directly toward the sun. They were far less distinct and separated, but looked much like a series of ripples on the surface of the snow, rather irregular, all of them scurrying as fast as possible, it seemed, toward the growing light. I should think their speed must have been twice that of the shadow bands before the eclipse. Unfortunately we did not notice the progress of these irregular shadows on the fence behind us, so I know nothing of the vertical component of their motion. There was very little surface wind at the time in any direction. I should think not enough where we were to explain the speed even in places entirely away from the fence. Of course, there may have been varying winds above. The ground was rather high on the ridge which runs northward from Newport about a mile from the center of the town. I have not seen yet any explanation of these phenomena which satisfies me, but I doubt if our very hurried observation (there were so many things to think about!) can be of much service.

I was sorry to see in the newspapers that you were hampered by clouds in Buffalo. I hope you got enough to repay your trouble. It would have been much better for science if you had had the wonderful opportunity that we had to see the eclipse under perfect conditions. It was indeed a wonderful sight.

I did not make a sketch of the eclipse partly because it looked so much like what was to be expected, partly because I saw only small prominences, and partly because paints seemed inadequate to represent it. The color of the corona to my eyes was almost pure white with perhaps a faint tinge of violet blue that may have been simply in contrast to the yellow light of the sun. The far yellower color of the sun was apparent at the moment of the last contact when the upper right hand edge of the sun blazed out in a color that looked almost golden in contrast to the paler corona.

With kind regards, Sincerely yours,

T. W. Richards.

P. S. The trouble with my timing of the duration of totality was my excitement and perturbation; as regards the time of incipient totality, the trouble was that I did not compare my watch soon enough with the Ar-

lington standard. My watch keeps very good time but is not infallible.

This account of the shadow bands, etc., has been assented to by the other members of our party—five in number.

This Newport, Rhode Island, observation of the January 24, 1925, eclipse was made at a point about 35 miles north of the center-line, or about 25 miles within the northern border of the path of totality which passed through Providence in the same state. In comparison, consider an observation from Scarsdale, New York, (near Yonkers), which was approximately the same distance south of the center-line. Alan N. Mann, with four companions, watched the eclipse from a position in front of his residence on Fox Meadow Road, and enclosed with his report a large scale surveyor's plat of the community, locating his house exactly and showing the compass points. Before totality, the bands progressed in a certain way across the front of his house, which I have projected and found to be towards the East (90°). (The top of a sheet of paper is treated as the house wall, and bands drawn on it accordingly. By folding the paper, and extending the bands beyond the fold, their "lay" and progress can be compared with compass directions.) After totality, the bands moved towards 55°, or about NE by E. Further, Mr. Mann wrote: "After totality, observation was also made on unbroken snow-field sloping up from observer towards sun at substantial angle. Lines radiated directly from sun and moved at right angles to their direction so that illusion was given of a movement like the spokes of a wheel."

One of the stumbling blocks to an understanding of bands is the casualness with which observers settle the compass directions. Every careful observer in the future should be equipped with a good compass and know how to use it. For example, Rev. W. Leigh Ribble recently contributed another band observation to the 1925 collection. He was, at that time, a student at the theological seminary at Alexandria, Virginia, near here, and saw bands before the eclipse maximum on the snow below his dormitory window. He named the building, located his room on a corner of the building to the left of the front door, second floor, and towards the sun, saying that, since the building faced north, the bands must have moved northeastward. This did not fit with my expectation; besides I thought I knew his campus situation to be different. (The reader can see the error from the above data.) So I drove over there next day with a compass, proved that his building faces east, and thus showed that the bands moved southeastward, as my theory demands.

However, at the 1932 eclipse, the first and only time I ever saw the bands, I was guilty of the same sort of mistake. Many people judge directions by the sun, but since the sun shifts through the year, error is probable. (In latitude 39°, the shift is 60° between June 21 and December 21.) After living at the address nearly a year, I had unconsciously concluded that the back of my house faced the south. Consequently, when I saw bands passing my back door from left to right as I faced it

from the yard (before maximum), and after maximum, moving away from it, I reported to *Science Service* that they had progressed, first towards the east and afterwards towards the south. Checking by the North Star and a compass months later, I found that the back of my house faces southwest, not south, and that the bands moved more nearly SSE before, and WSW afterwards.

When it is considered that many observers are located in positions new and strange to them, it is in the nature of things that very many reports of directions will be erroneous. On this account many reports cannot be taken too seriously unless observers have been forewarned to be careful. It may be said that the most satisfactory band observations have yet to be made, building on past experience.

The Eclipse of January 24, 1925

The eclipse of January 24, 1925, will be remembered as having had more witnesses than any other eclipse in history. This was due to the fact that the path of totality traversed one of the earth's most populous regions. In mid-eclipse at sunrise in northern Minnesota, the center-line passed near Grayling, Michigan; Buffalo, Binghampton, and Poughkeepsie, New York; Danbury and New Haven, Connecticut; then out to sea across Montauk Point on the eastern end of Long Island. From the center-line, the distance to either border of the path of totality was 40.6 miles in Longitude 94°, increasing to 55.8 miles in Longitude 70°. The northern border lay a few miles south of Syracuse, and passed between Cooperstown and Athens, New York, bisecting the city of Providence. The southern border lay about eight miles north of Jamestown, New York, about the same distance north of Stroudsburg, Pennsylvania, and crossed Manhattan Island approximately, if I remember right, at Thirty-Third street. On the 75th meridian, totality was in progress at 9:10 a.m., and the range of time over the region described was about seven minutes either way. The bearing of the sun at 9:10 A.M. for Latitude 40° to 42° averages close to 135°, or in other words almost exactly southeast.

Through the kindness of Dr. S. A. Mitchell, author of "Eclipses of the Sun," Dr. W. J. Humphreys, physicist of the Weather Bureau, loaned me at Easter-time, 1936, a bundle of reports of shadow band observations from this eclipse collected by the American Astronomical Union, the magazine "Scientific American," and the U. S. Weather Bureau. I catalogued these, the numbers running up to 109. There were several cases of duplication, however, where observers had reported to more than one agency, reducing it to 105. Professor Caroline E. Furness of Vassar turned in 21 reports from individuals of a physics class and two from visiting alumnae, all made for observations within a mile of each other. Of the total, only 79 reports yielded directions of any kind, and 19 described bands at pre-totality or at post-totality only. Two reports from 100 yards apart, describing band motion on walls, and which have not yet been reconciled, were considered to cancel each other.

There was added an observation at Norwich, Connecticut, by Rebecca Joslin, depicted by a drawing in her book, "Chasing Eclipses." Since then I have added the one from Alexandria, Virginia, far south of the track.

Using arrows centered on observers' sites, to indicate direction of wave-like band progress, (normals, perpendicular to band extension) the residue of band directions was laid down on tracing paper covering the special eclipse map printed by the Naval Observatory on the scale of approximately 40 miles to 1 inch. Separate plots were made for the pre-totality and post-totality reports. (A tabulation of these reports is provided for reference. See Part II.)

Following this, an ellipse, cut from cardboard to the shape of the moon's ground shadow, was placed upon the track-tracing, which varies between 2⅝ and 3 inches in width. (Scientific candor requires that I tell how I shaped my ellipse: I made a shape to lie on one of the azimuths which appear on the chart, and required the curves to meet the track boundaries. The outline was drawn free-handed.)

The ellipse was oriented so that its long axis agreed with the sun's azimuth, and on the pre-totality diagram was outlined the front edge of the ellipse, on the post-totality, the rear edge of the ellipse, these outlines being made where arrows lay thickest, so as to be most convenient for reference. (I circulated copies of these sheets to various astronomers.)

The results of the plotting may be summarized as follows:

Direction of band progress with respect to elliptical shadow spot	No. of locations reporting Pre-totality	Post-totality
Apparently perpendicular (or normal)	33	31
Apparently parallel	4	3
Other directions	5	7
Total locations	42	41
Total reports giving directions	73	51

Those recorded as parallel to edge of eclipse are thought to be reports of direction of extension, rather than direction of progress. Eleven out of the twelve arrows pointing in "other directions" than perpendicular or parallel with respect to edge of shadow, agree with their neighbors within 45°, leading to the belief that the observer was wrong as to compass directions.

Another way to summarize the arrows is on a geographical basis as in the table following.

These reports represent the band behavior from border to border of the track, with ten outside. They are not merely from the center-line where astronomers congregated. The observers included a bank president and his staff, who used a screen and movie camera for recording; seven or eight college professors and a couple of public school teachers, two M.I.T. students, Weather Bureau observers, a Navy lieutenant, several civil engineers, a Naval Observatory staff member, and other well qualified people. A large majority of the band direction reports are due to this high type of observer.

Pre-totality—	West of Bridgeport, Conn.	Bridgeport to Saybrook Jct., Conn.	Saybrook Jct. to Nantucket	Total
Out from shadow	16	..	..	16
In toward shadow	4	4	3	11
Extension only[1]	4	..	2	6
Parallel[2]	4	..	..	4
Other directions	3	..	2	5
	31	4	7	42
Post-totality—				
Out from shadow	2	3	..	5
In toward shadow	17	1	6	24
Extension only[1]	1	1	..	2
Parallel[2]	2	..	1	3
Other directions	4	1	2	7
	26	6	9	41

[1] Extension only, but motion perpendicular to edge of shadow.
[2] Parallel motion, that is, bands were reported to have progressed in a direction which was parallel to edge of shadow.

Whether viewed graphically in diagram form, or examined in tables as above, the data unmistakably indicate that:

(1) The bands were concentric with the elliptical shadow-spot;

(2) As far east as Bridgeport, the bands moved out from the shadow before totality, and in towards the shadow after totality;

(3) From Saybrook Junction to Nantucket, the movement was towards the shadow both before and afterwards;

(4) Between Bridgeport and Saybrook Junction, which differ in longitude by about one-half degree, it appears the bands moved towards the shadow preceding totality, away from shadow after totality.

At this point the writer broke off from preparation of this account to investigate several promising references he had encountered, and culled the following important investigation from Bulletin I, 1902, U. S. Weather Bureau, "Eclipse meteorology and allied problems," by Frank H. Bigelow, who held the position of professor of meteorology.

The eclipse of May 28, 1900, began in the Pacific, off the peninsula of Lower California and crossed the Southern States from New Orleans to Norfolk in about 25 minutes, totality having commenced at Virginia Beach at $8^h\ 50^m\ 30^s$, Eastern Standard Time. Through this region, the course of the shadow was about N 60° E, azimuth of the sun, N 95° E.

Professor Bigelow had distributed reporting forms to Weather Bureau observers and through them to volunteers, and received 33 reports which are tabulated in his account. Lancaster, South Carolina, yielded four reports, Raleigh, North Carolina, two. First reports gave information on width, separation, speed, and duration, besides extension or "lay" of the bands. By accident or design, a follow-up letter, *after* the reports came in, requested direction of progress and remarks. Professor Bige-

low states this avoided confusion as to extension and direction of progress.

The record of extensions before and after totality lacks completeness by eight items. Eighteen spaces are blank in the tabulation of direction of progress before and after. Employing a short line for band extension, and a direction arrow for band progress, Bigelow plotted all the residue on the edge of a circle representing the shadow, the pre-totality reports on the leading edge and post-totality reports behind, locating the respective symbols at their proportionate distances from the eclipse center-line.

Bigelow remarks: "In studying the direction of the bands on the circle, one receives the impression that they are (not only) more uniformly placed parallel to the same direction near the central part of the path both preceding and following, but that there is a tendency to confusion, or at least to a radical change in direction, for the stations located near the edge of the path." Later he writes: "We are, therefore, brought to conceive of the umbra as surrounded by semi-opaque *rings* in the plane sections, perpendicular to the axis." . . . and finishes with a further elaboration of his theory as to band origin.

The writer has traced the circle from Bigelow's diagram, laid down an ellipse in its place, shifted the arrows without altering their directions or distances from the center-line, and has tabulated results as follows:

Arrows showing direction of progress of bands	Before	After
Out from edge	19	7
In towards edge	3	15
Parallel with edge	1	1
	23	23

On two diagrams to a scale of one inch equals 40 miles, I have plotted separately, from the tabulated reports, the directions of progress at eight places in South Carolina for pre- and post-totality and find, on this larger scale, satisfactory agreement with the idea of an elliptical ring of bands, with a couple of observations excepted (i.e., two out of sixteen) which we are entitled to call "observational error."

Bigelow's account also contains a diagram of the eclipse track, scale about one inch equals 100 miles, which I have summarized as follows:

From state of	Total towns reporting	Bands†
Alabama	2	2
Georgia	3	2
South Carolina	7*	5
North Carolina	4	4
Totals	16	13

*Ignoring 2 out of 3 at Lancaster, S. C.

†Bands moved out from shadow before totality. Bands moved in towards shadow after totality.

The one and only report from Virginia was from Virginia Beach where the eclipse left the continent. This report showed that bands moved in towards the shadow before, and away from shadow afterwards,—a reversal.

Behavior at the edge of the shadow in the 1900 eclipse is typified by the report of Mr. Dry from Albemarle, North Carolina, which was exactly on the edge of the 50-mile wide track. He gave extensions as N 60° W and N 40° E before and after, respectively. The directions of progress, which must have been reported in a second letter, according to plan, were N 60° W and SW. His remarks illustrate the possibilities of ambiguity in this investigation: "The shadow bands moved to the northeast before totality, and to the southwest after totality. They began to flicker across the ground just before the second contact, coming from the direction indicated, and changed rapidly around by the west to the northeast without stopping." The reader is invited to try his hand at squaring the remarks with the directions first given!

The *Monthly Weather Review* of January, 1919, contains tabulated reports and a discussion of bands seen during the sunset eclipse of June 8, 1918, from the mouth of the Columbia river, through McAlester, Oklahoma. The treatment is by S. P. Fergusson, meteorologist, and refers to *Publications of the Astronomical Society of the Pacific* for August, 1918. Directions given are listed as "azimuth of bands," a term capable of ambiguous interpretation, in my opinion. However, from these data, I have prepared two charts for the two sets of band displays, and have compared the apparent extensions with the outlines of an elliptical shadow, oriented with its long axis pointing to the sun. Two-thirds of the 30 individual displays described lay definitely parallel with the edge of the ellipse, and most of the remainder were probably so, if the term "azimuth of bands" be interpreted as "azimuth of band progress," a matter of exactly 90°.

A. Lawrence Rotch of Blue Hill Meteorological Observatory (*Annals of Harvard College Observatory,* Vol. LVIII, Part III, 1908) has reported the results of an earnest investigation of bands at the eclipse of August 30, 1905, across Spain. The hearty coöperation of the Spanish government resulted in observations being secured from certain military balloons at Burgos, Spain, from which "white screens hung horizontally" . . . "In one of the balloons the bands were more clearly seen at the height of 3800 meters than on the ground, and were much finer (0.6 to 0.8 cm) and nearer together (1.2 to 1.6 cm), and had a velocity of 1.5 meters per second." (On the ground the widths reported varied from 1 to 10 cm, and distance apart from 3 to 25 cm.) The direction reports came from eight different points, through Spain and Majorca, to Tripoli, and Rotch wrote: "The observations at different places indicate that the bands lay nearly tangent to an ellipse having for its longest diameter the width of the shadow, and representing all points where totality occurred at the same instant."

As an observer in Russia, eclipse of August 19, 1887, Rotch says "clouds obscured the bands." In California, eclipse of January 1, 1889, seen in early afternoon with "a clear sky," Rotch says "bands were narrower and closer together than in previous observations, and were well defined. They were more prominent at high altitudes than at low levels."

I quote from Rotch, not only because he seemed ready to believe that the bands were concentric with shadow, but because he mentions two sets of observations disclosing greater clarity and distinctness of the bands at higher altitudes, in comparison with observations simultaneously obtained at lower points.

A miscellany of scattered reports from other eclipses than these, point to the same facts, namely, that the bands are in a ring form on the ground, and that the progress is either in towards the shadow or out from it, at right angles thereto.

Test of Hypothesis by Predictions

At Christmas-time, 1934, my sister called my attention to a few shadow-band observations quoted by Isabel Lewis in her "Handbook of Solar Eclipses," and to a brief section on the subject in Professor Mitchell's "Eclipses of the Sun," ninth edition. Four observations from these sources, one by Nolan from Lockyer's account of the 1898 eclipse, the diagram by Rebecca Joslin mentioned earlier, plus my own observation at Falls Church, and that of a fellow-teacher, Miss Maude C. Gunther, at Provincetown, Massachusetts, in 1932, provided the data for my development of a diffraction hypothesis to explain the bands. Details of this I read before the annual meeting of the American Association of Physics Teachers, at St. Louis, on December 30, 1935.

To test my hypothesis, the following February I prepared and issued to astronomical expeditions a sheet of predictions as to band directions from end to end of the eclipse track of June 19, 1936, across Asia. The predictions were for points in the center-line and 45°, measured along a circle with center on the center-line and diameter equal to the width of the eclipse path, out on either side of the center-line, for areas in the vicinity of Athens and Istanbul, Orenburg, Omsk, Tomsk, Stretnsk, Khabarovka, (Siberia), and Otchishi, (Japan). The author had underestimated the importance of the true shape of the changing shadow, and the band directions were predicted on the basis of a circular spot.

Dr. F. J. M. Stratton, leader of the Cambridge University party, sent in the observation of T. Sakurai of the National Research Council of Japan, made at Kamishari, Japan, on the center-line. On account of clouds, observation was possible before totality only. Bands moved from SE to NW,—that is, in the direction 315° approximately. My prediction had been 305° or the reverse. (Other data: Band width, 2 cm; separation, 3 cm; speed, 1 m/sec; display lasting 43 seconds. Wind from SW; cloud formations, "low cumulus forming on condensation.")

Thanks are due the Hon. Richard F. Boyce, American consul at Yokohama, for information on the eclipse track in Japan; and to the Hon.

Arthur Garrels, American Consul General at Tokyo, who sent clippings from Anglo-Japanese newspapers describing the eclipse; and to an unnamed gentleman connected with the British Embassy who travelled with the Oxford contingent and whom the Consul-General had interested in the project of making observations for me. The cloudy weather prevented an observation from this source.

Dr. J. A. Carroll, University of Aberdeen, in charge of a party at Omsk, Siberia, stated that, although he had four observers watching for the phenomenon, no bands were seen by them, and the Russian party there made no mention of having seen them. He added that "the atmosphere just before and during totality was extremely steady, no twinkling being observed, and the seeing and definition were amazingly good."

Located farther west at Ak Bulak, near Orenburg, Dr. Donald H. Menzel, leader of the Harvard-M.I.T. party, wrote that although he had asked several members of the party who were making radio studies, to look for them, "it so happens that none of them were able to observe any shadow bands at all. I myself was the only member of our party that observed them. About ten seconds before totality I turned and looked at the white tents of our expedition and saw some very faint bands moving approximately from east to west. The motion was not rapid, but I made no attempt to estimate exact velocity. I was too occupied with my other duties to attempt observations after the eclipse. The sky was cloudless and we had excellent seeing conditions."—My prediction for a center-line location (Orenburg) had been 67° or the reverse, 247°. The difference between the latter direction and Dr. Menzel's rough observation is 23° (taking his "approximately west" at exactly 270°).

Through the American consul-general at Istanbul, the Hon. M. K. Moorhead, who solicited the aid of Professor Scipio of Roberts College, a most complete report was forwarded from Dr. Alexis Dember, member of an Istanbul University party led by Professor Freundlich, and which observed the eclipse from Bilecik, Anatolia. Dr. Dember, who is studying this winter at California Institute of Technology, has informed me in a second letter, that Bilecik is the point of departure for the Turkish survey, and was 13'.5 (or miles, nearly) south of the center-line. The bands he reported were almost too faint to see and moved towards the SSE, changing to S, before totality; and towards SSE after totality. There were no clouds in any direction. The wind was from the SSW. My best prediction for band direction at this distance from center-line, would have been about E by N or the reverse, before; and S by W or reverse, afterwards. Therefore, I obtain no aid or comfort from this report. Dr. Dember has sent me two photographs of the site of observation, with directions marked, and band movement indicated, and which I have been able to check by means of the shadows of spectators appearing in the pictures. If the refractionists could make use of this observation I would be only too glad to turn it over to them. However,

and alas, the wind direction reported will only confound them; and it is of no use to say that only the surface wind was observed, for we have both the lower air refractionists and the upper air refractionists.

Through the kindness of the British Naval Commander-in-chief, Mediterranean station, bands were watched for on H.M.S. "Barham" at Alexandria, "Despatch" at Port Said, and "Arethusa" at Haifa. Reports from ships cruising on the track had been hoped for. These were 350 to 400 miles from the eclipse center-line, and their commanding officers, Captain Dodehouse, Captain Sarkson, and Lt. Comdr. Hill, reported no bands seen, though skies were clear. These reports at least suggest a limit beyond which bands need not be expected.

It was during the Easter vacation, 1936, a month after issuing my predictions for Asia that I received and worked through the 1925 reports. In the autumn I sought reports from Sydney, Australia, and Wellington, New Zealand, on the annular eclipse in December; but my requests were received late and no bands had been seen. (Recently I ran across at least one report describing bands at an annular eclipse.)

At Easter-time this year (1937) I prepared predictions for the Phoenix Islands and for Peru, also writing to Christmas and Fanning Islands which lay on opposite edges of the track midway between the foregoing areas.

Dr. John A. Fleming, director of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, requested Dr. Serge A. Korff, who was going to Peru for the eclipse, to collect observations for me from all parties contacted. Dr. Korff wrote me, stating that, although he looked for bands at the eclipse of June 8, he saw none, either before or after the eclipse. "Other observers reported that at other stations they were not seen either, with two exceptions. The reports of these I am enclosing."

In the absence of further word from Dr. Korff, I have concluded that these two observations are really the two appearances at one place, namely, the pre- and post-totality appearances. The observers were at Trujillo, Peru, midway between center-line and northern border of track, and the observations were made under the auspices of the Peruvian Eclipse Committee, San Marcos University. On my reporting form, he notes: "The sun was low in the sky, hence no bands visible on horizontal surface. Paper (see following paragraph) held vertically, at right angles to sun, showed"—(and his diagrams indicate bands moving down, in a diagonal direction toward lower right corner before totality, up towards upper right corner after totality). "Plane of paper points 25° E of N, since sun was at the moment 25° N of W. From this you can project for horizontal motion." My prediction had covered this position exactly and was 70°, or the reverse, 250°, before totality; and 340° or the reverse, 160°, after totality. The reader who will plot the reports against the predictions, will find complete agreement. He will note that the bands moved in towards the shadow, both before and after

totality, undergoing a change in direction within the brief period of totality ($2^m\ 37^s.3$ here) of about 90°, a very marked change indeed, and a hardship on the refraction idea. (The wind was not noted. It was clear with no stratified cloud formations. The bands before totality were 15-20 cm wide, separation 6-15 cm, speed 4 m/sec, display lasting about 120 seconds. Post-totality observations the same, except as to directions noted above.)

Not satisfied in regard to the "paper" on which the bands were seen, an inquiry to Peru has brought a letter from Dr. A. Rosenblatt, of Cracow, Poland, now professor in the science faculty, department of astronomy and geodesy of San Marcos University, Lima. The "paper" turns out to have been some "linen" 5.5 by 5 meters attached to the wall of "our house" at Trujillo. According to his diagram, this wall faces N 74° W (nearly), and the sun's bearing was N 65° W, or a direction nearly perpendicular to the surface observed. Sun's altitude is given as about 9° in the midst of eclipse. (Duration of totality given in preceding paragraph is derived from times of contacts, quoted by Professor Rosenblatt, and furnished apparently by Professor Yamamoto of the Japanese party, who gave the latitude and longitude.) The observations were made by five university students under Professor Rosenblatt's immediate supervision. He states that their drawing will be published in a special number of the *Revista de Ciencias,* a journal of the University.

Before leaving Peru, the latest acquisition must be reported. Dorothy A. Bennett, assistant curator, the Hayden planetarium, who mentioned bands in a POPULAR ASTRONOMY article describing their expedition to Cerro de Pasco, has just forwarded me the account of Mr. Charles H. Coles, chief photographer of the expedition. Mr. Coles writes: "Shortly before second contact, I went to the turret Eyemo. It was started on the appearance of the shadow-bands. The bands were approximately 2 inches wide, 8 to 10 inches apart, very ill-defined, moving rapidly from the lower left corner (north) to the upper right (south), and generally unsatisfactory for photography. In spite of the very poor and rapidly changing light, the camera was operated until totality." Professor Bennett adds that they were south of the central line of totality,—how far, I must inquire, but probably only a few miles, according to my rough plotting. If we assume that Mr. Cole's movie camera was directed at a screen or wall perpendicular to the ground and at the same time to the low-hanging sun, the bands which progressed from lower left to upper right must have been moving in over the ground from the northwesterly quadrant, in the general direction of southeast. Precise statement is impossible without more data. But southeast is exactly expressed as 135°. My predictions ranged from 115°, for a center-line position, to 175° for a position 45 miles south of the center-line! Note that whereas the bands moved in towards shadow both before and after totality at Trujillo, the pre-totality bands reported from Cerro de Pasco, two hundred

and fifty miles farther along, moved out from shadow.*

Finally I have to recount the observations at Canton Island and to compare them with my predictions furnished Dr. S. A. Mitchell, scientific leader of the Navy-National Geographic party, and who kindly consented to have a watch kept. These were perhaps the most carefully made observations as to compass directions, of any appearances for which I have predicted, because they were supervised by a Navy man who is always compass-conscious. As I do not hesitate to admit, none of my training or experience has equipped me to use the astronomers' data to the point of laying down the precise shape of the elliptical shadow-spot, as at Canton Island. Because the hour of the day would be nearly the same as that of the 1925 eclipse in the northeastern United States, for which I had an elliptical outline roughly prepared as previously told, I placed this outline on the track at Canton Island.

The course of the shadow-track here was about 63°, the azimuth of the sun 66°. I seem to have overlooked the 3 degrees difference in laying down the outline, but this is a small error. Tangents were drawn to the ellipse at points corresponding to 2nd and 3rd contacts for a point on the center-line, and for two points, each about 20 miles out on either side. For the northern location, now taken to have agreed with the position of Canton Island, a double-headed arrow pointed in the direction N 35° E, indicating the bands should go either in that direction or the reverse before totality. Another double-headed arrow, drawn normal to the tangent at the after edge of the eclipse-shadow diagram, was marked 275°, predicting progress in that direction or the reverse.

On the return of the expedition, Lyman J. Briggs, director of the National Bureau of Standards and chairman of the research committee of the National Geographic Society, released to me the following report of observations made under the direction of Capt. J. F. Hellweg, superintendent of the National, or Naval, Observatory. The reader is invited to note the extreme clarity and freedom from ambiguity attained in this report due to a repetition of each item in a different form.

"Captain Hellweg has kindly supplied me with the following notes: A smooth, flat, circular area filled with white sand was laid off and eight men were stationed around its border to observe the bands. At the beginning of totality the shadow bands appeared, but they were stationary except for slight tremors. They did not travel across the surface. These bands were lined in an east-west direction, that is, the bands themselves were parallel to an east-west line.

*Extract from letter from Assistant Curator Bennett, dated January 4:

"Your recent letter with additional questions about the shadow bands at Cerro de Pasco on June 8, has been received. Please find below the answers to your questions.

1. Mr. Cole's camera was aimed at a white sheet mounted on an almost vertical frame. Since the sun was about 7° high and the inclination of the screen not much more than that, the sun's rays which fell there were almost exactly perpendicular.
2. He saw the shadow bands only before totality.
3. He was approximately 40 miles south of the center line."

"At the end of totality, the bands appeared again. At this time they were moving rapidly with a velocity of translation of 10 ft. per second or more. The direction of motion was from east to west. The bands themselves were parallel to a north-south line, that is, they were at right angles to the bands which appeared at the beginning of totality.

"It should be added that Canton Island was not in the center of the path of totality, but displaced somewhat to the north of it."

What more could the tyro expect? Who knows but that the proper skill would shave a little off the nose of my ellipse so that the pre-totality error of 35 degrees would vanish? A small change in the curvature of an ellipse will make a great difference in a tangent and normal thereto. And the bands were stationary! My diffraction theory calls for stationary bands at certain moments in the eclipse. Such bands have occasionally been reported in the past, but now we have the word of a well-known scientist.

The direction of motion, after totality, was towards the west. West is exactly given as 270°. My prediction sheet, supplied to Dr. Mitchell, called for 275° at a point 20 miles north of the center-line.

Conclusion

The steps in my investigation have been as follows: (1) a personal observation of bands in 1932; (2) formation of an hypothesis, based on bands concentric with the shadow; (3) predictions for the eclipses of 1936 and 1937. (4) Meanwhile, I plotted the results of the 1925 eclipse and found apparent agreement with my hypothesis. (5) The results of Bigelow and Rotch have been examined and are found to support the idea of concentric bands.

Now, 2 out of 3 observers in 1936, and 3 out of 3 observers in 1937, have reported bands which were in substantial agreement with predictions based on my hypothesis. A candid world must therefore accept my statement that the shadow bands are in every case where carefully observed, parallel to the nearest edge of the shadow and consequently a portion of a complete ring of bands. Those who have supported the idea that the bands may be due to refraction by air waves, will surely be forced to abandon this position for obvious reasons.

Furthermore, the observations mentioned by Rotch, and the conclusions reached by Bigelow, point to the possibility that the bands come down through space.

The three modes of motion, namely, towards, away from, and stationary, with respect to shadow, are intriguing. What is next needed, in the way of observations, is a series of reports from points about 100 miles apart all along the track.

Who knows but that there may be found a nugget of golden truth at the end of one of these rainbow-arched eclipse tracks?

(Part II will consist of a discussion of views of other people, and the writer's diffraction theory.)

Roosevelt High School, Washington, D. C., September 7, 1937.

Some Meteorological Observations in Connection with the 1932 and 1934 Total Eclipses of the Sun

By WILLI M. COHN

Abstract. Data are presented of the temperature of the air, relative humidity, and barometric pressure before, during, and after the 1932 and 1934 total eclipses of the sun. Both eclipses were observed under perfect weather conditions. The data show pronounced effects of the cone of totality on the atmosphere.

Most eclipse observers in 1932 found the weather partly cloudy or cloudy during totality: see the publications by Brooks (1), Stratton (2), Thomson (3), and Lee and Bryant (4). The following note on some meteorological observations, although the data are incomplete, seems of interest because we observed the 1932 eclipse under perfectly clear skies. Some data are added for the 1934 eclipse, which also was observed under perfect conditions. It should be mentioned that the main program of our expeditions was a study of the polarization of the solar corona (5, 8), and that the meteorological observations could not be carried out as completely as desired because of lack of observers.

The 1932 eclipse station was at Small's Farm, near Gray, Maine, and the 1934 station at Laol Island of the Losap Islands group in the East Caroline Islands (South Seas). Our gratitude to Mr. and Mrs. A. J. Small of Gray and to the Imperial Japanese Government for the pleasant and profitable time spent at the eclipse stations was expressed in the preliminary report (5). Data for the eclipse stations and the eclipses are presented in Table I. The eclipse station at Small's farm was located on

TABLE I

ECLIPSE DATA

Date	August 31, 1932	February 14, 1934
Location	Small's Farm, Gray, Me.	Laol Island
Longitude	70° 21′ W (1)	—152° 44′ E (2)
Latitude	43° 53′ N	6° 53′ N
Elevation (approx.)	370 ft.	3 ft.
Apparent Altitude of Sun at Mid-totality	60° 40′	57° 55′
Time of totality, calculated	88ˢ	130ˢ (3)
observed	86ˢ	124ˢ
Times of contact, computed	E.S.T.	Local Civil Time (4)
I.	$2^h\ 20^m\ 24^s$	$8^h\ 52^m\ 24^s$
II.	3 29 02	10 16 20
III.	3 30 30	10 18 30
IV.	4 33 51	11 48 34

(1) Data obtained through courtesy of Dr. L. S. Barnes of Harvard College Observatory.

(2) Reduced according to observations by Dr. S. Nakano of Tokyo Astronomical Observatory. (6).

(3) Data obtained through courtesy of Dr. L. Watanabe of Kyoto Imperial University.

(4) Computed kindly by Messrs. C. Swanson and E. C. Bower of Astronomical Department, University of California.

a meadow close to the farm buildings, at the edge of town. The station at Laol was on sandy ground, open in S and SE towards the lagoon (inside of the atoll) and bordered on the other sides by palm trees covering most of the island.

Dry and wet bulb thermometers and two aneroid barometers were mounted in 1932 in the shadow of a small tree, about one foot from the ground. The 1934 arrangement was similar, the instruments being mounted about 3 feet from the ground, close to one of the tents of the author. All instruments were calibrated, after the eclipses, by means of standard instruments.

The meteorological conditions were as follows in 1932: The sky was perfectly clear during the entire eclipse, except that a thin sheet of alto-stratus appeared at the western horizon between the third and fourth contacts. (First contact = beginning of eclipse; second contact = beginning of totality; third contact = end of totality; fourth contact = end of eclipse.) The 1934 meteorological conditions were just as favorable as in 1932, no clouds being visible during totality; 34 minutes before, and 7 minutes after totality a few cumuli passed. Temperature of the air, relative humidity, and barometric pressure were determined for eclipse and comparison days. Corrected results for the eclipse days are presented in Table II.

TABLE II

METEOROLOGICAL OBSERVATIONS

Date	Time(x) P.M.	Thermometer—Wet Bulb in °F	Thermometer—Dry Bulb in °F	Relative Humidity in %	Barometer in mm.
August 31, 1932	1:25	73.7	81.4	70	754.3
	1:45	71.7	79.9	64	757.0
	1:56	73.2	80.9	70	756.7
	2:15	72.2	79.9	70	756.7
	2:22	71.7	78.9	71	756.1
	2:30	70.2	78.4	63	755.4
	2:45	72.7	79.9	72	755.4
	2:55	71.7	78.9	71	755.1
	2:57	71.7	77.4	76	.....
	3:00	70.7	76.3	76	754.8
	3:05	70.7	76.3	76	754.0
	3:08	70.2	75.8	76	.....
	3:10	70.2	75.3	78	.....
	3:16.5	69.7	74.2	80	.....
	3:19	69.7	73.3	84	.....
	3:22	68.7	73.1	80	.....
	3:24.5	68.7	72.3	84	.....
	3:26	68.7	71.3	88	.....
	3:27.3	68.7	71.3	88	.....
Totality	3:29-40"	68.7	71.3	88	.....
	3:30-20"	67.7	71.3	84	.....
	3:33	67.7	71.2	84	.....
	3:35.5	67.7	70.3	88	.....
	3:38	68.0	70.0	90	.....
	3:42	67.2	70.1	86	754.8
	3:45	67.2	69.8	88	.....
	3:47	67.2	69.8	88	.....
	3:50	67.0	69.8	86	.....

Date	Time(x) P.M.	Thermometer—Wet Bulb in °F	Thermometer—Dry Bulb in °F	Relative Humidity in %	Barometer in mm.
August 31, 1932	3:55	67.0	69.6	88	.....
	4:02	67.0	69.8	86	.....
	4:08	67.0	70.3	86	754.8
	4:15	67.7	71.8	82	754.2
	4:22	67.5	71.8	82	755.4
	4:30	68.2	73.3	78	755.1
	4:45	68.7	74.2	76	755.4
	4:55	67.7	72.3	80	755.6
	5:05	67.7	71.3	84	755.1
February 14, 1934	7:23	75.3	79.4	83	755.9
	8:21	77.3	81.2	83	757.4
	9:16	78.3	83.0	82	757.4
	10:01	78.3	81.7	86	757.4
	10:43	77.3	81.2	83	757.4
	11:16	79.3	84.7	78	757.4
	11:41	80.3	88.0	72	757.4
	12:21	81.3	88.0	76	757.2
	10:26.5(xx)	....	77.4	..	.....
	12:02 (xx)	....	85.2	..	.....

(x) Times in 1932 are Eastern Standard Time; in 1934, Local Civil Time.
(xx) Values of Dr. Ueta.

Temperature of the air and relative humidity. The temperatures indicated by the dry and wet bulb thermometers show for the comparison days steady curves according to the time of the day; the temperatures for the eclipse days show a decrease, starting between the first and second contacts, as seen in Figure 1. The minima were obtained 15 and 13

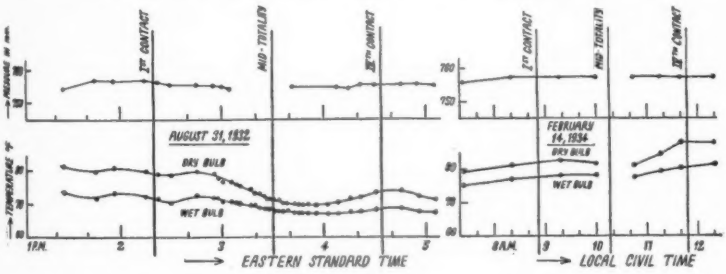


FIGURE 1
TEMPERATURE AND BAROMETRIC PRESSURE FOR THE 1932 AND 1934 ECLIPSES.

minutes *after* totality in 1932 and 1934, respectively. (The 1934 value was kindly communicated by Professor Dr. J. Ueta of Kyoto Imperial University who plans to publish his results later.) The relative humidity increased during the 1932 eclipse, with a maximum after totality. The trend of the temperature and humidity curves before and after totality in 1932 is very similar to that in 1934. The values are in agreement with data obtained at other eclipses.

Barometic pressure. Table II and Figure 1 show, shortly before the first contact, small increases of the barometric pressure, both in 1932 and 1934. A small decrease, followed by an increase of the pressure, occurred between the first and fourth contacts in 1932, whereas the (few) 1934 values seem to remain constant throughout the eclipse. The changes in barometric pressure in 1932 are of interest for investigations of the daily changes in connection with air mass analysis, etc., see, for example, Korselt (7). It must be kept in mind, however, that our pressure data are incomplete as stated above and do not include values for the total phases.

No accurate measurements of the velocity and direction of the wind could be made on these expeditions. It should be mentioned, however, that in 1932 the wind velocity was small at the time of the first contact. A cool breeze started 37 minutes after the first contact, and a fresh breeze from the E was present during the entire 1934 eclipse.

A careful study of the sky, right after totality and later, both in 1932 and 1934, showed no trace of haze or general cloudiness, such as might be expected from the cooling of the atmosphere. The good results of the ultra-violet exposures of the Harvard College Observatory party obtained in 1932 at Small's farm also argue against the presence of perceptible amounts of haze during totality.

The data show the influence of the cone of totality on the temperature and barometric pressure of the atmosphere. It seems desirable to investigate how far this influence extends from the cone of totality. Such work may be carried out rather frequently because of the comparatively great number of partial solar eclipses as against the small number of total eclipses.

Acknowledgments. My thanks are due to Mr. and Mrs. R. N. Mayall of Cambridge, Massachusetts, and to Mr. A. J. Small of Gray, Maine, for help in the observations. The 1932 expedition was partly supported by grants from the Rumford and Permanent Science Funds of the American Academy of Arts and Sciences. Further acknowledgments have been expressed in our paper (8).

LITERATURE

1. C. F. Brooks, *Bull. Amer. Meteorol. Soc.*, **13**, 159-160, 1932.
2. F. J. M. Stratton, *Monthly Notices R. Astron. Soc.*, **93**, 9-17, 1932.
3. A. Thomson, *Journ. R. Astron. Soc. Canada*, **27**, 32-33, 1933.
4. O. J. Lee and E. C. Bryant, *Ann. Dearborn Observ. Northwestern Univ.*, **4**, IV, 27-38, 1933.
5. W. M. Cohn, *Publ. Astron. Soc. Pacific*, **46**, 177-185, 1934.
6. S. Nakano, *Tokyo Astron. Obs. Reports*, **2**, 129-152, 1934; *Japan. Journ. Astron. and Geophys.*, **12**, 13, 1935.
7. E. Korselt, *Meteorolog. Zeits.*, **42**, 485-487, 1935.
8. W. M. Cohn, Polarization of the solar corona in the 1932 and 1934 total eclipses, to appear in *Astrophysical Journal.*

BERKELEY, CALIFORNIA, SEPTEMBER, 1937.

Notes from the Yerkes Observatory

By OTTO STRUVE

Assistant Professor Bengt Strömgren has been given leave of absence by the University of Chicago in order to enable him to accept a professorship in astrophysics at the University of Copenhagen. Professor Strömgren will retain his connection with the Yerkes Observatory and will coöperate in several research programs. He is primarily interested in the determination of the helium content of stellar interiors and of the hydrogen content of stellar atmospheres. He will also participate in the spectrophotometric work on faint Cepheid variables, which is now being carried on at the 24-inch reflector, and in the photometric work which has been planned for the 20-inch Schmidt camera now under construction.

Dr. Karl Wurm of the Astrophysical Observatory at Potsdam, Germany, has been appointed Visiting Assistant Professor at the Yerkes Observatory for one year, beginning April 1. Dr. Wurm will undertake several investigations dealing with band spectra in astronomical sources, and he will conduct several courses on modern astrophysical theories. Among these are: (1) Molecules in Stellar Atmospheres; (2) Some Planetary Emission Phenomena and Comets, (aurora, night sky, zodiacal light); and (3) the Atmospheres of the Planets.

Professor Frank E. Ross expects to return to the Yerkes Observatory in April, following a stay of six months at Pasadena. He will continue his investigations on the efficiency of photographic telescopes.

During the month of May, Dr. C. L. Pekeris of the Department of Physics, Massachusetts Institute of Technology, will lecture at the Yerkes Observatory on "Turbulence and Related Problems with Applications to Meteorology and Astrophysics." Dr. Pekeris has been appointed Research Associate during the Spring Quarter.

Beginning March 28, Professor Chandrasekhar will lecture on "The Mathematical Theory of Stellar Kinematics and Dynamics." During the Summer Quarter there will be two courses in addition to the symposium announced in the March issue of POPULAR ASTRONOMY. Dr. Wurm will lecture on band spectra, and Mr. E. L. McCarthy on "Optical Instruments of Astronomy."

In recognition of Professor Edwin Brant Frost's initiative in providing a spectacular ceremony at the opening of the Century of Progress Exposition in Chicago, the Executive Committee of the Exposition has donated to the Yerkes Observatory the amount of $4,800. The Yerkes Observatory is grateful to the Century of Progress Committee for this gift, and wishes especially to record its appreciation to Dr. Philip Fox who recommended the action. A part of the gift has already been used for the purchase of a registering microphotometer of the type designed

and used at Victoria by Dr. C. S. Beals. The instrument was made in the mechanical shops of Mr. Sidney S. Girling, Victoria, B. C., under the personal supervision of Dr. Beals. The remainder will be used for the construction of a 20-inch Schmidt camera, designed by Dr. Strömgren and intended primarily for accurate photometric work. It is expected that the Schmidt camera will be installed on Mount Locke.

Pending the completion of the 82-inch mirror for the McDonald Observatory, which is taking a disappointingly long time, a nebular spectrograph for the study of diffuse galactic nebulae has been installed on Mount Locke under the supervision of Professor Van Biesbroeck. The instrument consists of an equatorial mounting driven by a synchronous motor and carrying on one side a rectangular plane mirror, of 36 by 12 inches, and a slit, the width of which can be varied from one inch to about ten inches; on the other side the mounting carries an F: 1 Schmidt camera of 94 mm aperture, in front of which are mounted two large quartz prisms. A stationary plane mirror of 24 inches aperture is mounted on the continuation of the polar axis at a distance of 75 feet respectively from the slit and the camera. The spectrograph has thus an effective length of 150 feet. The camera is focused at the slit so that the instrument integrates over an area of 6′ diameter in the sky. This instrument, having two reflections, is only slightly faster than a similar spectrograph would be if attached at the Cassegrain focus of the 82-inch reflector. The gain consists in the elimination of the collimator lens. The principal advantage consists in the fact that the length of the slit corresponds to an angle of 1.5 degrees in the sky, so that large areas can be quickly examined for the existence of emission lines. Preliminary work has already led to the detection of fairly strong emission lines in various regions of Orion, some of which had not previously been known to contain definite traces of diffuse nebulosity. The most interesting of these regions is located several degrees northwest of λ Orionis in the vicinity of the 8.5 magnitude star BD +12°810. It is expected that the new spectrograph will greatly relieve the pressure for time at the 82-inch reflector. The program will include a study of emission lines in regions showing faint films of diffuse nebulosity on Ross's Milky Way photographs and even in regions of the Milky Way where direct photographs do not reveal such nebulosity; studies of the relative intensities of emission lines in galactic nebulae and determinations of the energy distribution in continuous spectra of galactic nebulae.

March 10, 1938.

Planet Notes for May, 1938

By R. S. ZUG

Note: Greenwich Civil Time is employed, unless otherwise stated. To obtain Eastern Standard Time subtract 5 hours; Central Standard Time, 6 hours; etc. The planetary phenomena are described as they are to be seen from latitude 45° N. The data are taken chiefly from the *American Ephemeris and Nautical Almanac.*

Sun. The positions of the sun for May 1 and May 31, respectively, are: $\alpha = 2^h\ 30^m.2$, $\delta = +14°\ 48'.5$; $\alpha = 4^h\ 48^m.7$, $\delta = +21°\ 47'.3$. The sun is in the constellation Aries until May 13, on which day it enters the constellation Taurus. Values for the equation of time are as follows:

Date 1938	Equation of Time (Mean - Apparent) m s	Date 1938	Equation of Time (Mean - Apparent) m s
May 4	— 3 11	May 20	— 3 40
8	— 3 32	24	— 3 24
12	— 3 43	28	— 3 1
16	— 3 46	June 1	— 2 29

Moon. Phenomena of the moon will occur as follows:

		h	m
New Moon	Apr. 30	5	28
First Quarter	May 6	21	24
Full Moon	14	8	39
Last Quarter	22	12	36
New Moon	29	14	0
Perigee	2	13	
Apogee	18	9	

Eclipses. A total eclipse of the sun occurs on May 29. The path of totality is over the south Atlantic Ocean. The eclipse is partial for observers in the southern portions of South America and Africa.

A total eclipse of the moon will occur May 14. Mid-totality occurs May 14, $8^h\ 43^m$, so that the entire eclipse will be visible over most of North and South America.

Further particulars concerning these eclipses are published in POPULAR ASTRONOMY, January, 1938, under *Planetary Phenomena in 1938* by Herbert C. Wilson.

Mercury. Mercury reached inferior conjunction April 21 and will be at greatest western elongation May 19, 14^h. The elongation will then be 25° 37'. When near greatest elongation the sunrise altitude of the planet will be about 11°.

Venus. Venus is now an evening star, setting $1^h\ 40^m$ after the sun on May 1, and $2^h\ 20^m$ after the sun on May 31. The stellar magnitude of Venus during May is about —3.4. The phase of the planet is gibbous, the planetary disc appearing 95 per cent illuminated on May 1, and 85 per cent illuminated on June 1.

The apparent angular diameter of the planetary disk on May 1 and June 1, respectively, will be 10".8 and 11".9, and the respective distances of Venus from the earth, for the same dates, 145 and 131 million miles.

On May 8, 0^h, (May 7, 6:00 P.M. Central Standard Time) Venus will be situated 2' to the north of Mars. The stellar magnitude of Mars at this time will be +1.5. Venus will therefore appear a hundred-fold brighter than Mars.

Mars. Mars is still an evening star, but is gradually approaching the sun in its apparent motion. By May 31 the planet's elongation will be only 17° and its apparent angular diameter 3".9. Its magnitude on that date will be +2.2.

Jupiter. Jupiter is a morning star, of magnitude —2.0. On May 31, it will be 1^h east of the meridian at sunrise, and at an altitude of 30°. Quadrature occurs May 22.

Saturn. Saturn is becoming more conspicuous as a morning star. By May 31 it will rise 2^h before the sun.

Uranus. Uranus is in conjunction with the sun May 4, 20^h.

Neptune. Neptune is well placed for observation during the spring months. Observers are again referred to *Planet Notes* for March, 1938, where a chart appears, illustrating the path of Neptune relative to the surrounding star field, for the period from March 1 to August 1.

Asteroid Notes

By HUGH S. RICE

Hermes

No further observations appear to have been made of this, the nearest of all known planetary bodies. It is therefore "lost" temporarily. Some important observations had been made by Jackson at Johannesburg, South Africa, but strange to say, these positions could not be reconciled with those made in Germany. In England, Crommelin, who has studied the matter, remarks that it is now discovered that the *reductions* of the "Jo'burg" positions were in error on account of a mistake of 25 years in the assumed equinox of the star positions. "The American computers," says Crommelin, "had guessed this 25 years precessional error, and verified it by cable. Once the Johannesburg places were known accurately, it was possible to get a good parallax by comparison with the Sonneberg photographs."

Considerable changes in the asteroid's orbit were made by perturbations caused by the close approach to the earth. *The shortest distance between the orbits of Hermes and the earth is 220,000 miles,* according to the calculations of Cunningham [referring to his second orbit as calculated]. This is *LESS* than the mean distance of the moon! The next apparition takes place in the spring of 1940, but the planet will not come as close as in 1937. The diameter is quite surely less than 1 mile. The mass is said to be of the order of 3,000,000,000 tons, according to Wood and Crommelin.

Hidalgo

On April 25 there comes to opposition one of the most exceptional of all the asteroids, 944 Hidalgo. It is unfortunate, but this object is beyond the reach of all but the largest telescopes, for its computed magnitude at this time is 18.3. It is interesting to note that at opposition it will be "near Theta Centauri." We quote the latter because of the Biela's Comet affair. Klinkerfues of Göttingen believed that this comet was near the earth and telegraphed to Pogson at Madras, "Biela touch earth November 27; search near Theta Centauri." This point was the antiradiant of the great meteor shower of 1872. A comet was actually found near the spot indicated, but it was probably not Biela's.

Hidalgo was discovered on October 31, 1920, by Baade, at Bergedorf, Germany. The exceptional features of Hidalgo refer to the orbit. About two years ago Vick made the latest refinements on the orbit, so that from his work, pub-

lished by the Astronomisches Rechen-Institut, we now give the latest data, which supersede those given in the textbooks. The major semi-axis of Hidalgo's orbit is 5.7999 astronomical units. The mean daily motion is only 254".027, from which we deduce the period of revolution of 13.9 years. This period of nearly 14 years is the longest at least of the well-known asteroids. Upon looking over the elements of over 1400 minor planets, we find that the next longest asteroid period is that of planet 884 Priamus. The mean daily motion of Priamus is 296".312, from which its period can be computed. Closely following this are 588 Achilles and 659 Nestor. Thus the group of planets called the Trojans appear to have the largest periods by far, of all the minor planets. For the perihelion distance of Hidalgo, we derive 1.9969 astronomical units; and for the aphelion distance, 9.6029 units. This aphelion distance is therefore greater than Saturn's mean distance [9.5388 units], so that Hidalgo goes out beyond Saturn's orbit. The eccentricity of the orbit is 0.6557; the inclination to the ecliptic plane, 42°.545. The asteroid does not approach closely to Saturn, on account of the high inclination, but it can come nearer to Jupiter.

Juno

Juno comes to opposition at the end of May, but will not be brighter than $10^{M}.0$. It can be picked up without much difficulty, according to positions as follows: On the evening of April 3, it is 4½° northeast of the star 20 Ophiuchi. Its northwest course takes it to a point about 6½° north of ζ Ophiuchi on May 27. While en route, it passes [May 3] just south of 23 Ophiuchi. At the latter time it will be in the finder field of Messier 10, "a rich globular cluster of compressed stars, on the Serpent-bearer's right hip"—according to Smyth's famous *Cycle of Celestial Objects*. The cluster is so easily resolved that Smyth was surprised that Messier said it "may be seen, with attention, by a telescope 3 feet in length."

Vesta

Not far away from Juno is Vesta, which is by far the easiest asteroid to observe this spring, as it is very bright, even reaching naked-eye brightness in May and June. On April 3 it will be not over 3° southwest of ζ Ophiuchi, and on May 9 just north of χ Scorpii, less than 1° away. The magnitude in this time changes from 6.4 to 6.0. From the data given, it can be found with the telescope and star atlas and without any special diagram of its apparent path.

There are two other rather bright minor planets which appear in the heavens at this time, 7 Iris, and 40 Harmonia. They can be observed with a 2-inch telescope. We give below their ephemerides, as computed by the Astronomisches Rechen-Institut. Iris is southwest of Spica, and Harmonia northeast of the same star.

Ephemerides of Asteroids. For 0^{h} U.T. Equinox of 1950.

	7 Iris ($9^{M}.5$)			40 Harmonia ($9^{M}.4$)		
		α	δ		α	δ
		h m	° ′		h m	° ′
Apr.	4	13 16.3	−15 55	Apr. 4	13 53.9	− 4 16
	12	13 8.8	−15 5	12	13 46.5	− 3 34
	20	13 1.6	−14 11	20	13 38.7	− 2 53
	28	12 54.9	−13 17	28	13 31.0	− 2 19
				May 6	13 24.1	− 1 55

Hayden Planetarium, American Museum of Natural History,
New York City, March 18, 1938.

Occultation Predictions

(Taken from the *American Ephemeris*)

Date 1938		Star	Mag.	IMMERSION Greenwich C.T.	*a*	*b*	Angle E from N	EMERSION Greenwich C.T.	*a*	*b*	Angle E from N
				h m	m	m	°	h m	m	m	°
Occultations Visible in Longitude +72° 30′, Latitude +42° 30′.											
May	7	14 Sex	6.3	22 35.0	−2.1	+0.8	88	23 42.8	−1.1	−2.0	330
	8	19 Sex	5.9	2 49.8	−1.0	−1.7	118	3 58.0	−0.7	−1.9	296
	9	p^3 Leo	6.2	2 45.7	−2.0	−0.4	75	3 40.4	−0.6	−2.7	341
	11	*i* Vir	5.6	22 37.0	−0.5	+0.8	110	23 44.8	−0.8	+0.3	301
Occultations Visible in Longitude +91° 0′, Latitude +40° 0′.											
May	8	19 Sex	5.9	2 34.3	−1.0	−2.2	147	3 41.7	−1.7	−1.2	272
	9	p^3 Leo	6.2	2 9.0	−1.9	−0.8	110	3 28.9	−1.4	−1.8	312
	15	51 G.Sco	6.4	9 11.6	−1.5	−0.8	85	10 30.6	−1.1	−1.4	274
Occultations Visible in Longitude +120° 0′, Latitude +36° 0′.											
May	5	162 B.Gem	5.6	4 28.9	−1.9	+0.8	49	4 59.5	+0.7	−3.8	351
	9	p^3 Leo	6.2	1 35.2	−0.6	−2.6	166	2 30.8	−2.6	+1.6	253
	12	550 B.Vir	5.8	2 25.3	−1.0	+0.8	101	3 35.2	−0.9	−0.4	312
	15	51 G.Sco	6.4	8 9.2	−2.4	+0.2	92	9 44.5	−2.4	−0.8	283
	16	116 B.Oph	6.3	6 24.6	−2.5	+3.4	43	7 12.8	−0.7	−1.7	333
	25	51 Psc	5.7	11 0.7	−0.2	+1.8	61	12 2.1	−0.6	+1.5	250

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

Comet Notes

By G. VAN BIESBROECK

There are no comets known to be observable at this time. Large telescopes may still reveal the presence of Comet 1935 *d* (Van Biesbroeck) but the brightness has probably dropped to about 18^M at this time.

In the morning sky two expected periodic comets should be hunted for: Comet 1925 II (Schwassmann-Wachmann) and Comet 1927 VI (Gale).

Ephemerides

1938	Comet Schwassmann-Wachmann α h m	δ ° ′	Comet Gale α h m	δ ° ′
Mar. 3	15 50.8	−30 16	18 21.7	−20 22
11	51.4	30 31	18 57.3	21 2
19	51.8	30 44	19 34.3	21 7
27	51.1	30 55	20 12.0	21 6
Apr. 4	49.7	31 3	20 49.5	20 27
12	47.5	31 8	21 26.0	19 24
20	44.8	31 9	22 0.9	18 1
28	41.4	31 6	22 33.8	16 23
May 6	15 37.7	−31 0	23 4.3	−14 38

Both are rather far south and probably faint. The first one deserves especial attention from the point of view of the rapid and large changes in brightness which it has shown repeatedly in the past. Its position calculated by Reverend Behrens (Circ. 707 of the International Union) is probably quite accurate. The location of the second one is much more uncertain but it is highly desirable that it should be picked up at this first return since the discovery in 1927. This ephemeris is continued from p. 59.

Williams Bay, Wisconsin, March 21, 1938.

VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

Changes in Period of RW Draconis: Messrs. Balazs and Detre of Budapest find from an investigation of light changes of the Cepheid variable RW Draconis, 163358, with a period of 0.44 days, that not only has the main period undergone a radical change, but also that there is some evidence of a secondary, smaller term period.

While the principal period, prior to 1906, can be well represented by a value of 0.442937 day, a mean value of 0.442811 day is required to represent the later observations. The authors find that the shorter term variation amounts to 41.64 days, and that it is accompanied by a change in the form of light curve, especially at maximum phase. The changes are comparable to those found in a similar type variable, RR Lyrae, and are in agreement with the results previously obtained by Blazko.

RY Ursae Majoris: The Me type, long-period variable, RY Ursae Majoris, 121561, with a total range of a magnitude, appears, according to C. Krebs of Berlin-Hemsdorf, to show evidence of a short-period term of 41.42 days superimposed on a long-period term of approximately 300 days. An examination of his plotted observations indicates a non-uniform light curve between successive cycles, with minima more sharply and better defined than maxima. When he derives the residuals from a mean curve drawn through the observations, he finds some evidence of a short-period term, although the mean visual range of light variation during this period is confined to approximately a tenth of a magnitude. Such a condition in a long-period variable of this type, if confirmed by future data, should prove of great interest to students of variable stars. Observations made with a photometer, capable of a high degree of accuracy, are greatly desired.

A New U Geminorum Type Variable: In *Harvard Bulletin* 857 Miss Swope announced that the variability of the star, now designated as FQ Scorpii, 170132, was of the long-period type, her conclusion being based upon the large observed range of magnitude. She has recently proved rather definitely that the star belongs to the U Geminorum, or SS Cygni type, with cycles varying from 25 to more than 80 days. The light curve presents many similarities to the curves of other U Geminorum stars, although little, if anything, is known about its characteristics at minimum, that is, when the star is below the sixteenth magnitude.

The star's position is identified as HV 4389 on Chart IV in *Harvard Bulletin* 887; although invisible there, it is located between two stars which serve well as comparison stars. The star is exceedingly faint at minimum, but it can be observed at maximum in the larger telescopes.

The Three Well-Known SS Cygni Stars: All of the three well-known SS Cygni type stars have been at maximum recently. SS Aurigae was very definitely rising on January 24, forty-six days following the previous rise, and remained at maximum for only a few days, being on the decrease to minimum again on January 27, after it had passed through what is known as a narrow-type maximum. Before SS Aurigae had reached its minimum, U Geminorum began to rise on January 28 and passed through a broad-type maximum at magnitude 8.9. The previously observed maximum of U Geminorum occurred late in October, 1937, and was of the narrow type. On February 27 SS Cygni was rising—one of its slower type increases—and passed through a broad-type maximum, number 298.

New Astrophysical Observatory in Georgia, Southern U.S.S.R.: The inauguration of the new Abastumani Astrophysical Observatory on Mount Kanobili, Georgia (Southern U.S.S.R.), is of interest to variable star observers because of the extensive program of variable star research which is planned, with particular attention to be paid to the W Ursae Majoris eclipsing stars and novae.

The location, at an elevation of approximately 5000 feet, is considered excellent for astronomical research. Construction was begun in 1934 and the present instrumental equipment consists of a 16-inch Zeiss refractor, a spectrohelioscope, an astrographic camera, and two reflectors. Their first *Bulletin* contains the results obtained for six eclipsing stars, including observations, light curves, and new elements.

Japanese Observations of Variable Stars, 1937: The observations of variable stars, as made by the members of the Astronomical Society of Japan during 1937, have just been issued. The total for the year is 6000, made by 18 observers on 265 stars, mainly of the long-period class. The observations are listed in bi-monthly installments. Observed dates of maxima and minima for 1936 for 38 stars are tabulated, as well as predicted dates for 103 stars for the year 1938. The society has been pursuing this branch of astronomical research since 1924, and during these years has accumulated over 40,000 observations.

AAVSO Observations, 1935-1937: With the publication of Number 8 of *Harvard Annals,* Vol. 104, the first Harvard volume of observations of variable stars, as made by the members of the American Association of Variable Star Observers, is completed. The observations include those communicated to the headquarters of the Association from November, 1935, to October, 1937. A total of 94,200 observations has been made by 235 observers on more than 500 stars, mostly of the long-period class, with a few special stars such as SS Cygni, R Coronae Borealis, novae, and R Scuti.

Twenty-nine observers made 66,000, or seven-tenths of all the observations contributed, one observer totaling more than 8000, another more than 6000, and seven between 3000 and 5000. Although 200 observers made less than 1000 observations each, they have played an important part in this program as is shown by an examination of the records. There are numerous instances where one or two observers, widely scattered in longitude, have obtained the sole evidence of the sudden rise or fall of a special variable, such as, for example, SS Cygni or RR Tauri.

This last compilation brings the total number of observations made by the AAVSO to the 600,000 mark. The reports for twenty-four years, 1911 to 1935, totaling 507,574 observations, appeared regularly in the columns of POPULAR ASTRONOMY and as annual reprints of the AAVSO. The observations for the next two years will appear in *Harvard Annals*, **107**, in the same form as in Volume 104.

Society of Friends of Astronomy: A recent publication of the Society of Friends of Astronomy, in Gorki, U.S.S.R., contains much material of interest regarding variable stars. One of the comparison stars for AY Geminorum (designated as SVS 782 Gem, $6^h\,23^m\,4^s$, $+19°\,44'$, 1855) has an observed amplitude of variation of $0^M.8$ ($10^M.2$ to $11^M.0$). From its color index the star is presumably of spectral type M, and probably of long period.

Also listed are 58 new variable stars discovered on photographs by the staff of the Sternberg Astronomical Institute in 1937. Small sketches of each field, showing the positions of the variable and the comparison stars, are included, thus making identification easy. Few stars brighter than the tenth magnitude appear on the published list. Although one particular area in Cassiopeia has been repeatedly, and successfully, examined for variables at several observatories, new variables continue to be found there. Curiously, no additional long-period variables were found in this particular region.

P. Parenago lists photographic observations of nine novae, mostly observed on Moscow plates taken twenty or thirty years ago. In the case of Nova Geminorum No. 1, the interval in days between invisibility and outburst has been reduced to three days, thus confirming the general impression that all novae undergo exceedingly rapid increases in magnitude during their early stages.

This particular issue is replete with additional data on recently discovered novae and with photometric observations on bright variables, including δ Cephei, η Aquilae, β Persei, and β Lyrae. The observations of these bright stars were made by N. Florja with the Rosenberg photometer at the Tashkent Observatory. The method consists of comparing the extra-focal image of the star with the reflecting surface of a Lummer-Brodhun prism by means of Nicol prisms, and the results appear to be of a high degree of accuracy, as evidenced by the published observations and light curves.

Observers and Observations, February, 1938: Miss M. Gausewitz of Milwaukee, Wisconsin, and Messrs. Essley and Skeer of the Case Astronomical Club of Cincinnati, Ohio, contributed to this report for the first time. Listed herewith are the names of observers, the number of variables observed, and the number of estimates made by each for the month of February, 1938. A total of 3707 observations by 55 observers is a good record as cloudy conditions prevailed at many observing stations.

Observer	Var.	Est.	Observer	Var.	Est.
Ahnert	37	94	Escalante	127	163
Blunck	5	5	Essley	3	5
Bouton	19	20	Evans	34	51
Brocchi	28	39	Fernald	3	3
Callum	28	47	Franklin	7	10
Cameron	5	11	Gausewitz	4	4
Chartier	7	13	Gregory	48	65
Cousins	22	118	Hartmann	126	207
Dafter	10	38	Herbig	78	193
Diedrich	6	12	Hiett	24	94
Ellis	19	26	Hildom	32	63

Observer	Var.	Est.	Observer	Var.	Est.
Holt	69	164	Peltier	80	103
Houghton	67	132	Peck	10	33
Houston	1	1	Purdy	4	4
Jones, E. H.	79	230	Rademacher	5	17
Jones, M. D.	2	4	Recinsky	3	9
Kearons	12	25	Rosebrugh	17	50
Kirkpatrick	37	147	Ryder	3	3
Knott	1	1	Schattle	2	18
de Kock	47	200	Sill	58	87
Kozowa	21	395	Skeer	3	4
Lizarraga	5	5	Smith, F. P.	17	17
Loreta	124	462	Smith, F. W.	2	6
McNabb	1	2	Smith, J. R.	4	4
Meek	19	86	Walton	5	12
Millard	27	51	Webb	44	44
Murphy	20	32	Woods	10	64
			Yamasaki	9	10

March 17, 1938.

A Note on a Cepheid Variable Star

The variable star H.V. 7380 was discovered by Miss Dorrit Hoffleit of Harvard on the plates of Harvard Milky Way Field 239. The position of the variable is R.A. $14^h\,07^m\,10^s$, Decl. $-44°\,25'.3$ (1900), (*H.B.* 902, page 14). In the course of a program involving measurement of the variable stars of Milky Way Field 239, the writer secured 136 estimates of the magnitude of the star on 13 plates of the MF series and 123 plates of the B series. The star is near one corner of the field, and on many of the B plates the star has an indistinct, hazy image, which is hard to measure for magnitude. The character of the star was unrecognized until Miss Henrietta Swope of Harvard Observatory called the attention of the writer to it. Then computations were begun by the writer with a view to finding the elements. The elements finally adopted were as follows:

Epoch	J.D. 2,425,771.300
Period	26.735 days
Maximum	$12^M.5$
Minimum	$14^M.3$

The magnitudes given are photographic. The light curve is characterized by a broad, flat maximum of about a week's duration. Cepheids with periods of this duration (about one month) are rather rare.

The writer wishes to thank Miss Henrietta H. Swope of Harvard Observatory for her advice and help.

Noah W. McLeod.

Christine, North Dakota, March 13, 1938.

Notes from Amateurs

Observations of Jupiter in 1937

I would like to submit, for the possible interest of the readers of Popular Astronomy, the following observations of the Great Red Spot of Jupiter made in 1937 by means of my 5-inch diaphragmed 7-inch reflector, with powers of 177× or 236×, the latter being rarely used.

Eye-estimated times of the transit of the Red Spot across the central meridian, from which the longitude of the Spot at the time could be worked out, were the

chief lines of study of the Spot. The period of this study was from June 15 to October 23, during which time 29 such observations were made on the center of the Spot and on the ends.

Below is the average longitude for each month:

	System II longitude
June	137°.7
July	141°.1
August	139°.9
September	134°.6
October	137°.5

This year (1937) the Spot lay in a very white South Tropical Zone with apparently nothing to accelerate or decelerate its motion of regularly decreasing longitude. However, various observers have reported an increasing longitude for both 1936 and 1937, though this is not confirmed by many.

The Spot was the most conspicuous mark on the planet, having a length (as observed here) of 23 degrees and a gray or brown color. In shape it was rounded on the north and flat on the south, almost touching the South Temperate Belt just to its south. Not much, if anything, could be seen of the Red Spot Hollow in the South Equatorial Belt.

These observations were part of a study of Jupiter in 1937 which included about 100 transit observations of various markings, 75 color observations, and 50 drawings.

HUGH M. JOHNSON.

3207 Cornell Street, Des Moines, Iowa, March 13, 1938.

The New Haven Amateur Astronomical Society

The New Haven Amateur Astronomical Society on Saturday evening, March 12, listened to a brief talk on the constellation Leo by Mr. Anyzeski and a report by Mrs. Rademacher on her observations of variable stars. Mr. Neale reported continued interest and activity in the Telescope Making Group, stating that the second richest field telescope has now been completed and that the group has been meeting regularly each month.

The proposed Constitution and By-Laws were read for the first time and discussed, action on them being deferred until the April meeting.

President Kimball then introduced the speaker of the evening, Dr. Arthur Bennett of Yale, who addressed the society on "The Aurora," giving a complete résumé of the phenomenon. He explained that the radiations are not concentric with North Pole or the Magnetic Pole but concentric with a central point considerably to the east and north of the Magnetic Pole near Greenland. Many early attempts were made to photograph the aurora but were largely unsuccessful as in 1892 the fastest plates required a 7 second exposure and obviously with an exposure of that duration the results were unsatisfactory and blurred and of rather uncertain value, whereas now plates exposed for ½ second clearly show well-defined shapes and characteristics of the aurora.

The method of determining heights of displays is the same as determining the parallax of a star and the speaker showed several projected photographs taken in Oslo, Norway, by Dr. Störmer of aurora with peculiar and unusual shapes. These were taken at three different stations, the first at Lillehammer, the second at Tomte, 81 miles south, and the third at Königsberg, 88 southwest. They showed the aurora to be 60 to 85 miles high and it was very interesting to note the apparent change in the position of the Pleiades in the background in relation to the aurora in these three photographs.

Some auroras have been measured to be as much as 600 miles high and some as low as 36 miles. The spectrum shows bands which have been identified as due to nitrogen and oxygen gas.

He explained the relation between sun-spot frequencies and the aurora. Dr. Bennett stated that the sun gave off charged particles which traveled toward the earth and encountered the magnetic lines of force circling out into space from the magnetic poles of the earth similar to the lines of force given off by a simple bar magnet, and through their influence caused the weird and beautiful auroras.

One photograph showed a distinctly marked fluted aurora, the flutes being 4 miles apart, about 4 miles wide, and 8 miles long. As is usual when addressing an amateur group, Dr. Bennett was called upon to answer many questions.

F. R. BURNHAM, *Secretary.*

Cleveland Astronomical Society

The regular annual dinner meeting again proved a great success with a record attendance. Eighty percent of our members were present including Ward Motts aged 13 the youngest member of our society. An excellent dinner was served, then Dr. N. T. Bobrovnikoff, Director of Perkins Observatory of Ohio State University and Ohio Wesleyan University, spoke on "The Atmosphere of the Planets." He called attention to the fact that here was a field of special interest to amateurs as being within the scope of moderate sized telescopes. In fact, the observations for magnitude of planets may require no optical aid whatever. While the great reflectors are used in the study of stars and nebulae the observation of planetary atmosphere has received little attention. When it is remembered that the discovery of the canals on Mars was made with an 8-inch refractor it is easy to believe that amateurs can accomplish much with telescopes available or constructed at home. Dr. Bobrovnikoff besides being a scientist has led an interesting life and is a hero of the World War, having been severely wounded in action. We hope to have him with us again at a later date. Distinguished guest present was Dr. Plaskett of the Dominion Observatory. We have two fine meetings promised in coming months. Dr. Shapley will be our speaker at one and Dr. Plaskett at the other. Those thinking of joining the society should do so at once to be sure of being present to hear these famous astronomers.

14 Lincoln Drive, Cleveland, Ohio. DON H. JOHNSTON.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

The last list of new members of the A.M.S. appeared about a year ago. We now give the names and addresses of those who have joined in the interval. During 1938 we hope to issue a new Bulletin containing a revised membership list, as the latter is a fluctuating group, with losses as well as gains. Let this opportunity be taken to say that, while we do not drop members, who are taking an active part in our program, because they are behind in their dues, still we cannot keep those who do nothing and in addition remain delinquent. The annual dues of $1 barely cover the cost of the reprints sent to each member and the postage used, and are lower than for most amateur societies. Were it not that the A.M.S. is supported

by the Flower Observatory it could not possibly function as it does. But the members should be willing to help by keeping up their dues, except of course the regional directors who are exempt by rule as they are all put to much personal expense in carrying out their duties.

LIST OF NEW MEMBERS

Salvator Bulla, 245 Troutman Street, Brooklyn, New York.
Don P. Grafflin, Y.M.C.A., Witherill Building, Greenwich, Connecticut.
Arthur P. Draper, Hayden Planetarium, New York City.
Professor Donald Faulkner, John B. Stetson University, Deland, Florida.
A. B. C. Darcy, 3008 University Avenue, San Diego, California.
H. A. Lower, 1032 Pennsylvania Avenue, San Diego, California.
C. A. Lower, 1032 Pennsylvania Avenue, San Diego, California.
Raymond Best, 221 E. Market Street, Tiffin, Ohio.
C. A. Federer, 43 W. 87th Street, New York City.
James J. Neale, 29 Fairmont Avenue, New Haven, Connecticut.
Mrs. E. S. Rademacher, 449 Pine Ridge, Hamden, Connecticut.
Herman Wobus, Airport Weather Bureau, Abilene, Texas.
H. A. Wark, 3626 Meadow Avenue, Station L, Cincinnati, Ohio.
G. H. Fentress, 2314 Blakemore Avenue, Nashville, Tennessee.

The main part of these Notes is taken up with full accounts of two very interesting Pennsylvania-New Jersey fireballs of 1936. Incidentally, we have reports on a very fine twilight fireball that appeared over North Carolina on 1938 February 7, about 6:00 P.M. One good additional observation may permit the calculation of its path. If anyone has such an observation, please send it in. We need observations of the splendid fireball of 1938 January 14, at 6:50 P.M. over Louisiana and a fine one that appeared over Maryland on 1938 February 3. In both cases we do not have enough data for a solution. Only by generous coöperation on the part of many people are we able to compute the path of the average fireball. Hence we are always glad to receive such an observation, as the last to come may be the key one for a solution, otherwise impossible.

The Pennsylvania-New Jersey Fireball of 1936 March 13/14

On the night of 1936 March 13/14, at 2:47:12 A.M., E.S.T., one of the most brilliant fireballs recently reported in America appeared above a point near Norristown, Pennsylvania, and, exploding west of Sandy Hook, New Jersey, apparently fell into the water, in two pieces, north or east of that place. No authentic fragment was recovered on land.

With the hearty coöperation of the press and radio, requests for observations were printed and broadcast, and about two hundred people responded. Questionnaires were sent to those most likely to give additional data of value. The numerical results follow:

Date	1936 March 13.82 G.M.T.	
Began over	λ 75° 44′	ϕ 40° 10′ at 144 km
Burst over	λ 74° 19′	ϕ 40° 23′ at 34 km
Ended at	λ 73° 53′	ϕ 40° 27′ at 0 km
Length of Path		215 km
Projected Length of Path		160 km
Sidereal Time at End Point		214° 40′
Duration	(7.3)	(13.0) sec
Observed Velocity	(29.5)	(16.5) km/sec
Radiant (uncorrected)	$a = 80° 30'$	$h = 42°$
Zenith Correction		2° 50′
Radiant (corrected)	$a = 80° 30'$	$h = 39° 10'$
	$\alpha = 161° 04'$	$\delta = 18° 13'$
	$\lambda = 155° 38'$	$\beta = 9° 27'$
Orbit	$i = 5°.7$	$\pi = 231°.6$
	$\Omega = 173°.6$	$q = 0°.76$

As usual most of the observers could not give angular estimates, but we were fortunate in having reports from several ships, from two surveying stations, from a few people who used instruments to determine their angles, and finally from some who plotted the fireball's path among the stars. The main complications were that most did not first see the object anywhere near its starting point, and some, particularly if very distant, confused the bursting point with the very end point. The solution therefore, as usual, depends upon selected data, carefully chosen. As we have evidence that fragments fell into the water, in the Lower Bay near Sandy Hook, we are justified in concluding that the fireball furnished meteorites. One of the definite reports is from the Scotland Lightship, the other from Captain H. P. Cooly of the tug O'Brien. The latter, who was passing West Bank Light, says ". . . towards the E.N.E. I saw a huge cloud of steam rising like a ghost from the water 400 feet away and felt the slight vibration of the rings of water as they sounded about the tug." Also an excellent observation from Major L. Dill, 18th Infantry, at Fort Wadsworth gave the altitude and azimuth as the object passed a window to the south, while the Scotland Lightship reported that the object passed to the north at an altitude of 70° to 80°. These fixed a point on the path only 5.4 km high. Three other points were measured from survey stations at Nodine and Round Hill, New York, and the path was plotted on a star-map by I. L. Meyer at Rutherford, New Jersey. These points determined the slope of the path. Some others agreed quite well, but these were the decisive observations. About 10 azimuth lines determined the beginning of the projected path; about 40 its end. The azimuth of the path itself was well checked by observers at Frederick, Maryland, Lancaster, Lewiston, Loysville, Port Kennedy, and York, Pennsylvania, and Princeton, New Jersey, who saw the path as exactly vertical or very nearly so. It seems unlikely that this azimuth could be more than 2° or 3° in error. As for the beginning point, its vertical height—144 km—was determined by using the line prolonged which fitted the selected points, mentioned above. The average from all 11 observations from distant ships, etc., is 153 km. But 144 km fits the three best observations from ships very well. Some of the ships gave their estimates of altitude to the nearest fifth degree only, also estimated altitudes are usually too high unless checked in some manner.

Sound

Sound phenomena were reported from over a large area, particularly near the end of the path. There is no doubt that the object "burst" near its end. Numerous observers specifically describe it. From the tug O'Brien and the Scotland Lightship which were very near its end point, it is described as consisting of "at least 10 large balls" and "balls of fire." By solving for the position of the point on the path which nearly corresponds to the first great noise, as timed at Princeton by J. S. Templeton, it is found to be 34 km over a point 7 km east of Spotswood, New Jersey. This was checked by estimates we have of the time-intervals from other stations and their distances from this point of explosion. In general they confirm the point chosen. It is not unreasonable to believe that this is the place where the original body (bodies??) broke into smaller pieces. As a good example, the account of Mrs. C. G. F. Gunter, who was writing in her home at Spotswood, New Jersey, when the fireball passed, may be quoted in part: ". . . suddenly it seemed that a crash occurred right overhead, my room seemed to shake visibly and there was a frightful roar. The others were now also awakened and could still hear the rumbling detonations rhythmatically moving as if in tremendous waves." At Nodine, New York, 70 km distant from explosion point "A rumbling noise was heard which lasted about 10 seconds." Professor Baldwin

Smith at Princeton reported: "The noise began with an extremely sharp, heavy explosion, with a long rumbling following." Even as far off as Swarthmore, Pennsylvania, Mrs. J. H. Willetts reported: "Perhaps less than ten minutes afterwards the house vibrated with a dull rumbling, as if to a far off explosion." These extracts prove that there was a first sharp report, followed by loud and long rumblings. As said above, I believe that the "bursting" of the meteor furnished the first sound and the rest was the usual shock-wave.

Light

The color of the meteor seems on overwhelming evidence to have been bluish-white. The blue tinge was not only reported by most who saw the object, but also by many who, being indoors, saw only its light. This refers to it before or at bursting. After that the numerous pieces are recorded in general as being red, the meteor more yellow. Many observers say that the tail was as wide as the meteor's diameter and was at least ten times as long. Some described it as being of but one color, others as being several at once. No long-enduring train was left, The apparent size of the meteor naturally varied, according to the observer's distance. The estimates of high weight run from one-sixth the moon's diameter to its full diameter. For observers near the path it was probably about 20'. As to the brightness, there is the usual wide range of opinion. However, as the quarter moon was shining, that the meteor's light attracted such wide attention, even with people indoors, is positive proof that it considerably exceeded moonlight. We have direct estimates from "very brilliant" to ten times that of the full moon. However, in this matter we have an observation of real importance, made by a man specially trained in the estimation of the intensity of illuminations. He is P. R. Bassett, Vice-president in charge of Engineering of the Sperry Gyroscope Co., who was on a train 2 or 3 miles east of Jamaica, Long Island. Part of his report is quoted: "I have made many studies . . . of visibility under full moonlight so that I can state quite positively that the illumination was considerably brighter than full moonlight. The visibility of trees at a distance of several thousand feet under the conditions of an interfering glare from street lights, etc., has caused me to place the estimated intensity at at least 0.2 foot candles. This is approximately ten times the illumination of the full moon. From probable height and the newspaper reports as to location, I have figured the slant range from my location to the meteor was approximately 30 miles . . . (then) I have arrived at . . . about 6½ billion candle power. (Note. The writer gets 5.01 billion for this distance.) . . . Therefore, figuring on the basis of the black body temperature being 3500 C, it is possible to obtain from the intrinsic brilliancy the area of the light source and consequently the area of the meteor. A black body of 3500 C gives approximately 200 candle power per square mm brilliancy . . . therefore, the diameter of the meteorite was approximately 18 feet. Of course some of this area might have been flame and extended over a larger surface than the solid body. Also, the color may depart very much from incandescent temperature due to actual burning . . . I have not figured in anything for atmospheric absorption . . ."

I have calculated that Mr. Bassett was 42.8 miles from the bursting point. On this basis and using all his assumptions as given above, the radius comes out 4.0 meters and the candle power 10.2×10^9. It is not meant to infer that the solid masses had any such cross-section but this seems a most reasonable estimate for the radius of the accompanying gas-cap. It may at once be objected that Millman's work on meteoric spectra indicates they are bright-line and not continuous, the temperature being from 1400° K to 3300° K. Hence the black-body assump-

tion is at best a first approximation, nevertheless the results are of qualitative interest.

Orbit

Not unless the geocentric (i.e., corrected observed) velocity is known can the orbit of a fireball be computed. In this case the duration based on the average of 29 estimates was 7.3 seconds, but unfortunately the one observer, E. J. Newman at Rahwah, New Jersey, who checked his estimate later, using a stop-watch, got 13 seconds. There is no way to pass judgment on which is the more probable. Under these circumstances it seems best to calculate the usual parabolic orbit which gives the perihelion distance and inclination nearly enough for most purposes. The elements show that the former is large, the latter small. From this fact, and its low observed velocity we may infer an elliptical orbit and that the body was a permanent member of the Solar System. We find no fireball with a radiant near that of this object in the Von Niessl-Hoffmeister Catalogue.

Our most hearty thanks are due to both press and radio as well as to the two hundred people who so kindly sent in reports, in some cases several, which make this paper possible. Lack of space prevents acknowledgment by name, except in specially important cases.

Fireball of 1936 August 23

A bright daylight fireball appeared on the afternoon of 1936 August 23 at 6:21 E.S.T., not long before sunset. It started over the Maryland-Pennsylvania border and, crossing New Jersey, ended over the Atlantic, east of Cape May. Reports were received from about a hundred people, thanks largely to the prompt action of Mr. I. Levitt, of the Fels Planetarium, who put requests for observations in the papers, the writer being out of town. As no stars were visible, it was most fortunate that the moon was in the S.S.W. and that for many observers the fireball passed under that body. But for that, the path would be far more uncertain than it is. The object itself was described generally as bluish-green or bluish-white, elongated in shape, and leaving a relatively short train of sparks behind it. There was no long enduring train and no explosion; it simply died out suddenly, high in the air. The fireball appeared as perhaps one-sixth the moon's angular diameter, as seen by the average observer. Unfortunately people situated under the path did not see it or at least did not report it, nor did any west of the beginning point. It was mostly seen by people well to the north or south. In general, they reported its path as exactly horizontal or with a small slope. Such apparent slope as there was could be readily explained by the greater distance of the end point. The character of the observations, hampered as they were by daylight, precludes an exact solution. Due to the moon's favorable position, the fireball's height as it passed is the best determined point on the path. This turns out to be 10 km lower than the calculated end point. Due to the greater weight of the former determination, the better procedure would be to consider the path as horizontal and the 10 km simply an error of observation. I have made this somewhat dangerous assumption in order to calculate the radiant. The duration, based upon 28 estimates (all from memory) is 4.7 seconds, but as several of these referred to only part of the path, 5 seconds is computed. This, however, is the minimum observed duration. As the fireball overtook the earth, this gives a geocentric velocity probably too great, remembering that the path lay wholly at a moderate height. As the fireball had certainly suffered some retardation before this height was reached, it is useless to compute an orbit using this observed velocity as a datum.

The following results were found:

Date	1936 August 23.47 G.M.T.
Began over	$\lambda = 76° 10'$; $\phi = +39° 42'$
Ended over	$\lambda = 74° 33'$; $\phi = +38° 57'$
Sidereal time at end point	248°
Average height (horizontal path)	49 km
End height	(59 km)
Length of path	162 km
Velocity-geocentric	32 km/sec
Radiant	Azi. 148°; Alt. 0°; R.A. 110°; Decl. +42°

Flower Observatory of the University of Pennsylvania,
Upper Darby, Pennsylvania, 1938 February 25.

A Peculiar Hole Near Tiffin, Iowa

Second Paper

By C. C. WYLIE

In *Contributions of the University of Iowa Observatory*, Number 8, page 265, POPULAR ASTRONOMY, **45**, 445, we published a description of a peculiar hole found in the bottom lands of Clear Creek, near Tiffin, Iowa. A contour map of the hole, and a map showing the location with respect to the creek, were published. Tradition calls this hole a "buffalo wallow," but in the opinion of university men most familiar with the buffalo, or American bison, the hole was not made by that animal. Experts in hydraulic engineering, who personally examined the hole, said that it was not produced by the ordinary processes of stream flow and erosion. Experts in geology also examined the hole, but could offer no explanation of its origin.

The possibility that it might be a sinkhole was considered. However, bedrock is more than 100 feet below the bottom of this hole. If the underlying rock did crack, it appears that the channelling of the waters would be from the creek bed and not from comparatively high land on the flood plain. The possibility that a jam of driftwood had caused it was considered. However, a jam of driftwood in the stream occurred while we were investigating the hole. It caused some change in the channel, but a year later there was no noticeable depression. The silt had filled in around the driftwood leaving the flood plain level.

The possibility of an artificial origin was considered, but the hole does not have that appearance, and there is no reason for excavating such a hole in the bottom land near a stream. Certainly it would be of no value as a reservoir of water, since it is dry except when filled by flood water, and the nearby creek is never dry.

The possibility of a meteoric origin was considered. The elliptical shape, with a rim to the south, is similar to known meteoric craters. Soil borings from the rim of the hole contained reddish layers with plenty of iron oxide. Borings in the flood plain surrounding the hole showed no such layers of concentrated iron oxide. This is in agreement with the meteoric theory. Probings showed that the gravel under the hole is at about the same level as the gravel in the bottom of Clear Creek. This is against the meteoric theory.

Since the publication of the preceding article, tests of the soil borings for iron and nickel have been made by Professor Louis J. Waldbauer of our Chemistry Department. The tests showed plenty of iron, even in the borings from the flood plain a considerable distance from the hole; but they showed no nickel. The ab-

ence of nickel is against the meteoric theory.* If a meteoric hole were very old, the nickel might be dissolved, but from the erosion, we believe the age is less than one hundred years.

Settlements within ten miles of this hole were establsihed between 1835 and 1840, and a newspaper has been published since 1840. If the hole had been formed by an explosion, meteoric or other, windows would have been broken ten miles away. Since we have no record of such an explosion, we know that such a meteor did not fall later than 1840. The appearance of the hole makes it pretty certain that it was not formed as long ago as 1840. Hence, the hole can hardly have been made by a meteor.

Following the publication of this article, Messrs. J. D. Boon and C. C. Albritton, Jr., of Southern Methodist University, referred us to "Torrential Flood Erosion in the Connecticut Valley in 1936," published in the *American Journal of Science*, **34**, 293, October, 1937, by R. F. Collins and M. Schalk. This article by Collins and Schalk appeared in the same month as our article on the Tiffin hole. It describes a number of "swirl-pits" excavated in the bottom lands of the Connecticut River at the time of a powerful flood. Some of the larger swirl-pits are very similar to the Tiffin hole in size, in shape, and in location with respect to the stream. Messrs. Boon and Albritton suggested, and we agreed, that the swirl-pit theory for the origin of the Tiffin hole appeared better than anything previously suggested.

In favor of the swirl-pit theory is the size and general appearance of the hole, the location with respect to the stream, and the fact that Clear Creek overflows the bottom land rather frequently. It does not account for the reddish layers in the rim of the hole, but there is plenty of iron oxide in the bottom lands. The only obvious objection is the fact that there is a noticeable rim on three sides of the Tiffin hole. The Connecticut swirl-pits showed no such rim. But the flood plain near the Tiffin hole is now covered with underbrush. In such a case it seems possible that soil thrown out by a swirl might be caught by the underbrush, and a rim formed in that way.

SUMMARY

On the basis of the present information, it appears highly improbable that the Tiffin hole is a "buffalo wallow," an old oxbow loop, a sinkhole, a pit from driftwood, a man-made hole, or a meteoric crater.

The "swirl-pit" theory fits the observed facts fairly well. The hole probably was made at the time of some powerful flood of Clear Creek fifty or sixty years ago.

University of Iowa, March 19, 1938.

*Mr. John Davis Buddhue discusses this point more fully in the following article. He agrees with us that the Tiffin hole is probably a swirl pit.

Chemical Tests of "Oxide" from the Tiffin Hole

By JOHN DAVIS BUDDHUE

After reading Dr. C. C. Wylie's article on the Tiffin, Iowa, hole[1] it occurred to me that an examination for nickel of the "oxide" described by him might furnish additional evidence for, or against, a meteoritic origin of the hole.

Accordingly, I obtained a sample of the reddish-brown soil through the courtesy of Mr. G. M. Ludwig. This was extracted with acids and tested for nickel

[1] POPULAR ASTRONOMY, **45**, 445 (1937).

with dimethyl glyoxime in the usual way, but with a negative result. Iron, on the other hand, was abundant, as it is in some soils. On communicating my findings to Dr. Wylie I found that Professor Louis J. Waldbauer, of the Chemistry Department of the University of Iowa, had already examined three samples of soil. He also found no nickel, but plenty of iron. One of his samples contained "layers of oxide," and came from a place just inside of the rim, and at a depth of between two and three feet. This contained less iron than the other two, which came from some distance outside of the hole and contained no layers of "oxide."

Inasmuch as no *bona fide* meteorite is known that does not contain some nickel, these tests indicate two possibilities, (1) the hole was not made by a meteorite, and (2) the hole was made by a meteorite but the nickel has subsequently been removed. The possibility that the hole was made by a nickel-free meteorite seems hardly worth considering.

It seems to me that since nickel forms only 7 to 8 percent of most meteoritic iron, there is some possibility that it might all have been removed. Factors favoring this hypothesis are the rather finely divided condition of the so-called oxide of the Tiffin hole, and the fact that "the sand and gravel below the hole are continuously saturated with water." Moisture, possibly assisted by dissolved substances, is, of course, the most probable, if not the only reasonable method by which the nickel could have been removed.

In an unpublished paper read before the 1937 meeting of the Society for Research on Meteorites, I have shown that about half of the analyzed rusts from iron meteorites show a decrease in the nickel content as compared with the unaltered metal. Of the remainder, however, some show an increase in the nickel content. When duplicate analyses are available, some may show an increase, and some a decrease in nickel content. (This paper may eventually be published in Contributions of the Society for Research on Meteorites, in this journal.)

About two years ago Dr. A. K. Harris examined the site where the Willamette, Oregon, meteorite was discovered, and found that a layer of "iron shale" or rust had been left behind.[2] Dr. Harris kindly furnished me with a lump of rock that had been in contact with the lower surface of the oxide.

This rock was a very fine grained, probably igneous rock, that had been altered to a considerable depth to a chalky substance. The whole sample was quite porous and more or less impregnated with oxides. In the interior I found some green specks which I thought might be zaratite, or other nickel mineral. However, they were insoluble even in aqua regia, and hence were neither zaratite, nor any known nickel hydroxide, or carbonate. Tests were made for nickel on both the outer and interior portions of this rock, but none was found. The overlying oxide itself shows a decrease in nickel of 1.15 percent, as compared with the mean of two analyses of the unaltered metal.

H. H. Nininger has remarked upon an even greater decrease in the nickel content of the oxide from the Brenham, Kansas, pallasite.[3]

It follows from these observations, that the rust derived from meteorites may contain less nickel than the corresponding metal. The case of the rock from beneath the oxide of the Willamette meteorite shows that the nickel oxides, hydroxides, or whatever they may be, are at least sometimes more soluble than the iron oxides; otherwise nickel should have been found in the rock. The nickel cannot

[2] *The Mineralogist*, July, 1936, p. 10.
[3] *Transactions Kansas Academy of Science*, **32**, 63 (1929).

have been retained in the primary oxide, because that itself shows less than the original metal.

Consequently, the absence of nickel in the four analyses of soil from the Tiffin hole, does not necessarily constitute proof that the hole was not made by the impact of a meteorite.

Whether 100 years, which seems to be the maximum age ascribable to the hole, would be sufficient for the removal of all the nickel, is a question that the available data cannot answer. However, the fine-grained condition of the so-called oxide, and the abundant moisture would presumably favor the removal of the nickel, if there ever was any to be removed.

99 South Raymond Avenue, Pasadena, California, February 6, 1938.

Contributions of the Society for Research on Meteorites

Edited by FREDERICK C. LEONARD,
Department of Astronomy, University of California at Los Angeles

President of the Society: H. H. NININGER, Colorado Museum of Natural History and American Meteorite Laboratory, Denver
Secretary of the Society: ROBERT W. WEBB, Department of Geology, University of California at Los Angeles

The Great-Circle Distribution of the Tektites*

By LINCOLN LA PAZ,
Department of Mathematics, The Ohio State University, Columbus

Introduction.—The statement is frequently made that there is no positive evidence for the meteoritic origin of the tektites. However, the constancy in the radioactive contents of tektites coming from such distant parts of the world as Europe, Asia, and Australia, recently reported by Dubey,[1] suggests their genetic relationship, and hence, in view of their peculiar distribution around the globe, would seem to be interpretable as positive evidence of their common cosmic origin. The discovery by Weinschenk[2] of a fusion crust in the case of the Kuttenberg tektites is definitely positive evidence of the meteoritic nature of these specimens.[3] It is the purpose of the present paper to present further evidence[4] supporting the hypothesis that tektites are of meteoritic origin, *i.e.* are either meteorites, or, as L. J. Spencer has supposed,[5] are the congealed form of the molten spray produced by crater-forming meteoritic impacts. In 1927-28, E. David and his associates[6] in Australia, and, independently, K. de Boer[7] in Germany, pointed out a surprising regularity in the distribution of the then known tektite areas, in that all such areas were found to be situated on, or near, a great circle of the earth's surface. The position of this great circle was only roughly described, but, as is evident from a recent paper of Lacroix,[8] knowledge of even its approximate location has been of much service in suggesting further areas in which search for tektites should be made; *e.g.* search for and discovery of the tektites of Indo-China resulted from knowledge of the existence of the David-de Boer great circle. If the meteoritic origin of the tektites is denied, the occurrence of such a tektite locus—even when

*Read as "Meteorite Distribution Problem I," at the Fifth Annual Meeting, June, 1937.

it is delineated as a *band* 20° wide as David sketched it—seems very improbable. The question is: How improbable? The first object of the present paper is to give a quantitative answer to this question. The second object is to raise and answer a similar question for tektite and silica-glass centers discovered in the last three years and not lying near the David-de Boer great circle. A final object of the paper is to draw certain inferences from the nature and location of the tektite loci themselves.

§1. *Mathematical Preliminaries.*—On the basis of the fundamental notions of continuous probability, it is possible to prove the following lemmas relating to random distributions of points on a zone Z of a sphere.[9]

LEMMA 1. If $p_1(Z)$ denotes the probability for a point P taken at random in a zone Z of area $A(Z)$ to fall in a *fixed* α-cap in Z, then

$$p_1(Z) = [2\pi(1 - \cos\alpha)/A(Z)]. \tag{1.1}$$

Evidently $p_1(Z)$ is an upper bound for the probability that P falls in a fixed α-cap on the sphere, and hence $p_1(Z)$ is an *upper bound* for the probability that the arc-distance between two points taken at random in Z does not exceed α. Hence:

LEMMA 2. If $p_{2,\alpha}(Z)$ denotes the probability for two points taken at random on a zone Z of area $A(Z)$ to fall in an α-cap, then

$$p_{2,\alpha}(Z) \leqq [2\pi(1 - \cos 2\alpha)/A(Z)]. \tag{1.2}$$

From this last result we deduce at once

LEMMA 3. If $p_{n,\alpha}(Z)$ denotes the probability for n points taken at random in a zone Z of area $A(Z)$ to fall in an α-cap, then

$$p_{n,\alpha}(Z) \leqq [2\pi(1 - \cos 2\alpha)/A(Z)]^{n-1}. \tag{1.3}$$

In particular, if Z is a β-zone,[10] then the quantity $A(Z)$ appearing in the foregoing lemmas is given by

$$A(Z) = 2\pi \sin\beta. \tag{1.4}$$

When Z is a β-zone, we shall denote $p_{n,\alpha}(Z)$ by $p(n, \alpha, \beta)$ so that by (1.3) and (1.4) we have

$$p(n, \alpha, \beta) \leqq [(1 - \cos 2\alpha)/\sin\beta]^{n-1}. \tag{1.5}$$

Let R be any region on the sphere which can be approximately represented by use of combinations of spherical triangles, quadrilaterals, and caps. Consider pairs of distinct points, P_1, P_2, taken at random in R. Any such pair suffices to determine a great circle of the sphere. In later sections we shall face the problem of determining an upper bound for the probability that two or more such great circles, taken at random, approximately coincide. In order to solve this problem it is expedient to make use of a transformation similar to one already employed by the author.[11] Suppose that P_1 and P_2 are two distinct points on any great circle C of the sphere. If a direction, say that from P_1 to P_2, is chosen on C, then the pole P of C lying to the *right* of the directed great circle is uniquely determined, and, conversely, if such a pole point is given, the corresponding directed great circle is uniquely determined. Hence, there is a bi-unique correspondence between directed great circles through point pairs (P_1, P_2) and poles P, and we may accordingly choose the poles P to represent the corresponding great circles. Evidently problems relating to the approximate coincidence of great-circle arcs in R, are by this device transformed into problems relating to the approximate identity of the pole points corresponding to the great circles. In order to solve the problems which thus arise, we observe that, under our transformation, the region R is mapped into an-

other region $\boldsymbol{R}$ of the spherical surface, namely the locus of the pole points P of the directed great-circle arcs P_1P_2 corresponding to all possible choices of P_1 and P_2 in R. If we denote the area of $\boldsymbol{R}$ by $\boldsymbol{A}$, then the probability that a pole point P taken at random in $\boldsymbol{R}$ falls in a specified subregion of $\boldsymbol{R}$ is a fraction involving $\boldsymbol{A}$ in its denominator. Since it is always possible to find a zone Z of the sphere with area $A(Z) = \boldsymbol{A}$, we may replace $\boldsymbol{R}$ by Z. Our original problems are in this fashion replaced by equivalent problems formulated for a zone Z to which lemmas 1, 2, and 3 are applicable. Finally, if we deliberately choose Z so that $A(Z) < \boldsymbol{A}$, our results for the zone-problems will provide upper bounds for the probabilities relating to the original problems as formulated in $\boldsymbol{R}$.

§2. *The David-de Boer Locus.*—An upper bound on the probability of the fortuitous occurrence of the David-de Boer locus will be determined in this section by use of lemma 3 of §1. If the meteoritic origin of the tektites is denied, then it appears that the tektites must be regarded as of either volcanic or fulguritic[12] origin. The former possibility seems definitely ruled out by geologic and petrographic evidence. In the latter case, since the geographical distribution of lightning strokes in the turmoil of a moving thunderstorm is essentially a random one, widely separated occurrences of tektites must be regarded as independent events produced by an agency acting at random over that part R of the land area of the globe which was subject to thunderstorms at the times (Tertiary and Quaternary) when the tektites were formed.

Consider now such pairs of centers of tektite areas as the Billitonite center, P_{11} : (108° E, —3°), and the Schonite center, P_{12} : (15° E, 60°); the South-Australite center, P_{21} : (130° E, —28°), and the Kuttenberg center, P_{22} : (15° 10′ E, 50°); the Tasmanian Darwin-Glass center, P_{31} : (145° 30′ E, —42°), and the Moldavite center, P_{32} : (15° 10′ E, 49° 5′); the Tasmanian Australite center, P_{41} : (145° E, —40° 50′), and the Malayan center, P_{42} : (102° 30′ E, 2° 30′); the Coolgardie center, P_{51} : (122° E, —31° 30′), and the Bangka center, P_{52} : (106° 20′ E, —2° 30′); and, finally, the Northwest-Australite center, P_{61} : (124° 30′ E, —22° 15′), and the Siamese center, P_{62} : (104° E, 16°).[13] Each of the pairs of points (P_{i1}, P_{i2}), $i = 1, 2, \ldots, 6$, suffices to determine a great circle on the surface of the earth. In view of our assumption that the tektites are distributed at random over R, the twelve points P_{ij}, $j = 1, 2$, and therefore the six great circles determined by the pairs (P_{i1}, P_{i2}) are random in R. Our problem is to determine an upper bound for the probability of the approximate coincidence of these six random great circles. In view of the transformation described in the preceding section, this problem is equivalent to that of determining an upper bound, B, for the probability, p, of the approximate identity of the six pole points P_i corresponding to the directed great-circle arcs $P_{i1}P_{i2}$. From the coördinates of the points (P_{i1}, P_{i2}) of the various pairs, it is easy to compute the longitudes and latitudes of these corresponding pole points P_i. It is found that the six pole points P_1 : (163° 40′ W, 29° 40′ N), P_2 : (166° 30′ W, 40° N), P_3 : (174° 40′ W, 40° 30′ N), P_4 : (165° 40′ W, 37° N), P_5 : (164° 50′W, 25° 20′ N), P_6 : (157° 30′ W, 27° N)[14] thus determined, all lie in a spherical cap of inradius 10° with incenter O_1 at (166° W, 34° N). Now if the six great-circle arcs $P_{i1}P_{i2}$ were random in R, the six pole points P_i would be random in the image $\boldsymbol{R}$ of R defined in §1. Hence the probability p that these six points would exhibit the observed closeness of grouping, can be found by calculating the probability that six points taken at random in $\boldsymbol{R}$ all fall in an α-cap with $\alpha = 10°$. In order to be able to use the simple formula of lemma 3 we shall replace $\boldsymbol{R}$ by a zone Z as explained in §1. We shall take for

Z a β-zone. Since we do not know the precise extent of the land area R subject to thunderstorms in Tertiary and Quaternary times, the area $\boldsymbol{A}$ is not exactly known. We do not, therefore, attempt to determine β so that $A(Z) = \boldsymbol{A}$, but seek only to find a β-zone for which $A(Z) < \boldsymbol{A}$; for, on computing an upper bound for the probability P that six points taken at random in such a zone all fall in an α-cap with $\alpha = 10°$, we shall automatically secure an upper bound B for $p < P$.

In evaluating β we make reference to geologic evidence indicating that essentially the same land area was exposed in Tertiary and Quaternary times as now[15] and that periods when the world enjoyed a warm, moist climate favoring thunderstorm development[16] were of much longer duration than those intervals, such as the Recent, when conditions favorable to thunderstorms occurred only in certain zones.[17] Presumably, therefore, the present land area subject to thunderstorms is of smaller extent than the corresponding region R of Tertiary and Quaternary times. With this assumption in mind, even a cursory examination of a globe will convince the reader that for, say, $\beta = 25°$, $A(Z)$ is much less than $\boldsymbol{A}$. But for this choice of β, the probability P referred to previously has, by (1.5), with $n = 6$, $\alpha = 10°$, and $\beta = 25°$, the value

$$P = p(6, 10°, 25°) \leqq [(1 - \cos 20°)/\sin 25°]^5 < 0.00006 = B.$$

In view of the hypotheses under which B was obtained, a moment's reflection will show that $p < P$ must be really very much less than 0.00006.

§3. *The David-de Boer Great Circle.*—We have seen that the incenter O_1 of the α-cap in which all six of the pole points P_i lie, has the coördinates (166° W, 34° N). The equation of the great circle G_1 with pole O_1 is, therefore,

$$\cos 34° \cos \phi \, [\cos (166° - L)] + \sin 34° \sin \phi = 0, \tag{3.1}$$

where L denotes longitude measured positively west from Greenwich and where ϕ denotes latitude. We shall refer to G_1 as the David-de Boer great circle, since it is found to represent satisfactorily the trend of the band locus described in the introduction.

There is little accord between G_1 and the great circle determined, *e.g.* by the Indo-Chinese tektite center and the Philippine center. A possible explanation of this fact, based on the supposition that the tektites under consideration are of meteoritic origin, will be given later. Here we wish to point out merely that, in accordance with the familiar theorem of compound probability, to assume that centers such as those in Indo-China and the Philippines represent occurrences independent of the centers P_{ij} employed in §2 can be shown to lead to a probability for the fulguritic origin of the tektites of the David-de Boer band still *smaller* than the probability p of §2.

On the basis of the introductory remarks in §2 and the results obtained up to this point, it appears that the odds are more than 16,600 to 1 that the tektites found in the areas under consideration are of meteoritic origin; *i.e.* that they either resulted from collisions between the earth and the components of a long-drawn-out swarm of glassy meteorites similar in general character, if not in chemical composition, to the Canadian meteoritic procession of 1913 February 9, but on a larger scale, or are the remnants of the molten spray produced when a chain of meteorite craters was formed by a series of meteoritic impacts distributed on, or near, the David-de Boer great circle.

David and many others who advocate a meteoritic origin for the tektites, assume that the collisions which produced these bodies occurred almost simultaneously.[18] On the other hand, evidence has been adduced by F. E. Suess and others, notably R. Janoschek,[19] indicating that the tektites under consideration are the re-

sult of two sequences of collisions separated by an interval extending from the Helvetian period of the Miocene epoch of the Tertiary (Moldavite fall) to the Pleistocene epoch of the Quaternary (Australite fall). If Janoschek's conclusions are accepted, it becomes necessary to provide an explanation for the occurrence of two (or possibly more) tektite falls widely separated in geologic time but in every case distributed along the David-de Boer great circle, G_1. The most immediate explanation would seem to be that the cosmic mass or masses whence the tektites came moved in an orbit about the earth lying in the plane of the terrestrial equator and that throughout part of Tertiary and Quaternary times the trace of this plane on the surface of the earth was near the great circle G_1.[30] The band-like nature of the David-de Boer locus might then be explained by assuming the occurrence of slight oscillations of the equatorial plane about a mean position and by reference to the width of the stream of cosmic matter impinging onto the earth. This view is supported on the one hand by G. H. Darwin's theory of an earth-moon system in which, initially, the moon was very near the earth and moved about it in the plane of the terrestrial equator,[31] and on the other by evidence indicating that in earlier geologic time the equatorial plane of the earth *did* lie near the plane of the great circle G_1.[32] The chief objections to the foregoing explanation are, first, the inadequacy of known forces to produce the displacement of the pole from its assumed position in Tertiary and Quaternary times to its present position;[33] second, the existence of a northeastward extension of the David-de Boer locus, to which attention has been called by Lacroix; and, third, the recent discovery of tektites on the Ivory Coast in an area remote from G_1. An alternative explanation of the David-de Boer locus, which avoids all but the first of the objections just enumerated, will be given at the end of the next section.

To conclude this section, we observe that if the longitude of a point on G_1 is assigned, we can compute the corresponding latitude from (3.1). Hence, we can specify precisely the location of areas in which, conceivably, tektites should be searched for entirely around the globe. Since throughout much of its length the circle G_1 passes over the water, there are long stretches of ocean floor from which deposits brought up by dredging should be carefully scrutinized for tektites. The discovery of these bodies in such deposits from parts of the ocean floor which have been submerged since the early Tertiary period would at once disprove the fulguritic hypothesis and show that Spencer's theory of their origin also is untenable. Finally, it may be noted that the David-de Boer circle can be shown to pass approximately 2° south of the meteorite craters in Estonia, the same distance south of the Colombite center, and therefore almost midway between the Nicaraguan tektite center and the neighboring places of find of the supposed tektites of Paucartambo and Macusani in Peru, and approximately 6° south and west of the Henbury meteorite craters in Australia.

If in the determination of G_1 we had given weight to the recently discovered tektite centers in South China, French Indo-China, and the Philippines, rather than exclusively to the older centers of the David-de Boer band which all lie near the southwest edge of the area now known to be sprinkled with tektites, then the resultant great circle would have passed even closer to the meteorite craters in Estonia and Australia and the Colombite center. This curious alignment of known meteorite craters along the David-de Boer great circle suggests that Spencer's conjecture as to their origin may be correct.

§4. *The Lacroix-Spencer Great Circle.*—Only two recognized tektite or silica-glass deposits fail to lie in the David-de Boer band or in what Lacroix refers to as the northeastward extension of this locus, including the Philippines. These de-

posits are, first, the tektites of the Ivory Coast, described in 1934 by Lacroix, and, second, the silica glass of the Sand Sea of the Libyan Desert examined by L. J. Spencer in December, 1934.

Consider now the following pairs of points: P_{11} : (25° 34′ E, 25° 18′ N), P_{12} : (4° W, 6° 45′ N); P_{21} : (1° 25′ W, 6° 30′ N), P_{22} : (61° 45′ W, 27° 30′ S). Here P_{11} is the point where, according to Spencer, the largest amount of silica glass was found in the Sand Sea, P_{12} is the midpoint of the line joining Dimbrokro and Assikasso, villages on the Ivory Coast near which, according to Lacroix, tektites have been found, P_{21} is the supposed meteorite crater in Ashanti which contains Lake Bosumtwi,[24] and P_{22} is the approximate center of the Campo del Cielo meteorite craters in Argentina. Denote by P_1 and P_2 the poles of the great circles determined by these pairs of points. Then it can be shown that P_1 : (103° W, 52° 50′ N) falls in a small circle C of inradius $\alpha = 4°$ described about P_2 : (101° 10′ W, 56° N) as center. Now if the tektites of the Ivory Coast and the silica glass of the Sand Sea were of fulguritic origin, the great circle determined by the pair (P_{11}, P_{12}) would be random in the region R of §2 and hence the pole P_1 of this circle would be random in the image $\boldsymbol{R}$ of R. But, then, by use of lemma 1, §1, and an argument paralleling that of §2, it could be shown that the probability p for the pole P_1 to fall in the fixed α-cap, C, with center determined by the entirely independent events which produced the craters at P_{21} and P_{22} is less than 0.006. Since we have used only one of many independent Sand Sea-Ivory Coast pairs, we infer that p is actually much less than 0.006 and hence that it is highly probable that the deposits at P_{11} and P_{12} are *not* of fulguritic origin. In what follows we shall suppose that these deposits are of meteoritic origin.

Denote by O_3 the midpoint of the arc joining P_1 and P_2. The equation of the great circle G_3 with pole O_3 is

$$\cos 54° 30′ \cos \phi \, [\cos (102° - L)] + \sin 54° 30′ \sin \phi = 0. \tag{4.1}$$

We shall refer to G_3 as the Lacroix-Spencer great circle. The circle G_3, like G_1, is characterized by the same curious alignment of tektite areas and meteorite craters. Presumably we may expect further discoveries of both tektites and craters, along the Lacroix-Spencer great circle. To my knowledge attention has not heretofore been directed to the existence of this second tektite-meteorite-crater locus.

From the equation (4.1) it can be shown that the directed great circle G_3 comes into the meridian at Lake Bosumtwi from the direction N 55° 30′ E, while Maclaren, from the distribution of débris about the crater, estimated that the meteorite producing the crater came in from approximately NNE.

Finally, we note that the angle between G_1 and G_3 is 48° 10′. Let us denote by π_1 the terrestrial equatorial plane at the time the various meteoritic masses under consideration fell onto the earth and by π_2 the plane bisecting the dihedral angle between the planes of G_1 and G_2. If we assume the approximate coincidence of π_1 and π_2 and if we suppose that the cosmic masses previously referred to moved about the earth in orbits lying in a plane inclined to the old equatorial plane of the earth by about the same angle as that between the ecliptic and the present equatorial plane, then it is easy to explain the existence of the two distinct tektite-meteorite-crater loci G_1 and G_2. Furthermore, it is evident that arcs of such great-circle loci might occur anywhere within a zone Z of the earth's surface bisected by π_1 and having a width of about 48°. We have thus an explanation, not only of the David-de Boer band but also of its northeastward extension and the Lacroix-Spencer locus. This explanation therefore escapes all but one of the objections raised in §3. It also will be noted by reference to a globe that many large land areas are

outside of the tektite zone, *Z*. *E.g.* this condition is true of the United States. We have accordingly a simple explanation of the fact that, as yet, no tektites have been reported from an area as large and as intensively farmed as this country.[25] The obvious paleo-climatological implications of the assumption that π_3 was once the equatorial plane of the earth are consonant with geologic observations.

References and Footnotes

[1] Dubey, V. S., *Nature*, **132** (1933), 678.

[2] Weinschenk, E., *Centralblatt für Mineralogie, Geologie, und Paläontologie* (1908), 737-42.

[3] On the other hand, absence of such a crust can scarcely be regarded as proof of non-meteoritic origin, for the fusion crust where originally present may have disappeared under the action of those corrosive agencies which have effectively sculptured even the chemically resistant glass of the tektites.

[4] For the most part this evidence is derived by use of the calculus of probability. Evidence secured in this manner has been admitted at the scientific bar since the time of Laplace. An interesting recent example is the calculation by means of which H. Jeffreys has shown the improbability of the suggestion that the occurrence of a land and a water hemisphere on the earth is due to asymmetry in the distribution of infalling planetesimals. *Cf. The Earth*, 2nd Ed. (1929), 314-15.

[5] Spencer, L. J., *Nature*, **131** (1933), 117.

[6] David, T. W. E., Summers, H. S., and Ampt, G. A., *Proc. Roy. Soc. Victoria*, **39**, (N.S.) (1927), Pt. II, 167.

[7] De Boer, K., *Astr. Nach.*, **234** (1929), 135.

[8] Lacroix, A., *Comp. Rend. Acad. Sci. Paris*, **199** (1934), Pt. II, 1539-42.

[9] In the statement of these lemmas the term *α-cap* is used to signify the smaller region bounded by a circle of inradius $\alpha < \pi/2$ drawn on the surface of a sphere.

[10] A β-zone is a zone of width β, one of whose base planes passes through the center of the sphere.

[11] La Paz, L., *Astr. Nach.*, **246** (1932), 201-4.

[12] The tektites will be said to be of fulguritic origin if they are fusion products resulting from lightning strokes which either penetrate into the earth or pass through dense dust clouds above the earth.

[13] The coördinates of the points P_{ij} are based on data taken from the papers of E. Weinschenk, A. Lacroix, and E. David *et al.*, referred to in earlier footnotes. The choice of pairs has been guided chiefly by the desire to secure a set of great circles conforming to the general trend of the David-de Boer locus, but this matter was not the sole consideration; *e.g.* the ultra-acid glasses, Moldavite and Darwin glass, were paired.

[14] Calculated coördinates, here and elsewhere, are given to only the nearest 10′.

[15] Pirsson, L. V., and Schuchert, C., *Text-book of Geology*, 464 and Chap. LII.

[16] Humphreys, W. J., *Physics of the Air*, 2nd Ed. (1929), Chap. 17.

[17] Pirsson and Schuchert, *loc. cit.*, [15], 984-5.

[18] See, *e.g.* Jeffreys, *loc. cit.*, [4], 185.

[19] Janoschek, R., *Akad. Wiss., Wien, Math.-Naturw. Kl. (Akad. Anzeiger*, No. 17, June 28, 1934).

[20] *Cf.* in this connection the conjectures of G. Linck, *Centralblatt für Mineralogie, Geologie, und Paläontologie*, (1934), Abt. A, **15**.

[21] Darwin, G. H., *Scientific Papers*, **2**, 101 *et sqq.*

[22] Kober, L., "Gebirgsbau der Erde," *Handwörterbuch der Naturwiss.*, 2nd Ed. (1934), **4B**, 776. Milankovitch, M., "Der Mechanismus der Polverlagerungen und die daraus sich ergebenden Polbahnkurven," *Gerland's Beiträge zur Geophysik*, **42** (1934), 70-97.

[23] Jeffreys, H., *The Earth*, 2nd Ed. (1929), 304 *et sqq.*

[24] Maclaren, M., *Geog. Jour.*, **78** (1931), 270-6.

[25] Tektites are exhibited occasionally in placer mining camps in the United States. However, it is the author's experience that persistent questioning discloses always that such specimens came originally from the tektite-sprinkled gold fields of Australia.

Communications and Comments

Under this heading we shall publish from time to time such material as does not properly fall under any of the established headings of this journal. Here, too, may be found, when occasion arises, articles which the editors may not be willing to give sanction to but which, nevertheless, may be provocative of thought along new lines.

Comets, Their Origin and Behavior

Let us assume that we have a great cloud, consisting of gases of various density and some small, solid particles, and let it be out in space too far from the sun, we will say, to be affected by the sun's gravitation. The cloud will form into a sphere under the influence of the gravitation of its own substance. The solid particles will be in the center, and the denser gases will surround these in the order of their densities, the rarest gases forming the outer layer.

Now let us bring the spherical cloud into the region of the sun, say a few billion miles from the sun. It will start to move toward the sun in response to that body's gravitational force. The spherical cloud will move toward the sun just as any solid body would move so far as velocity and acceleration of the entire cloud are concerned.

The question arises, will the spherical gaseous body remain in a spherical shape as it moves toward the sun as a falling body? To answer this question, let us investigate the problem in the light of the law of gravitation and the laws of moving bodies. Call the spherical cloud "the cloud."

The acceleration and the velocity at any time may be computed from the known distance from the sun (and the known travel) of the center of the cloud. But a particle on the near side will have greater gravitational force toward the sun than a particle at the center of the cloud. Hence this particle will be urged with greater force and greater velocity toward the sun than the cloud as a whole. A particle on the far side of the cloud will be subject to less gravitational force toward the sun than the cloud as a whole, hence it will tend to lag behind. Every particle in the near half will be subject to greater force toward the sun than the particles in the far half. This means that the cloud will be elongated, for while the gravitation of the cloud itself acts to hold the cloud spherical when no outside influence is exerted, it cannot maintain the spherical shape against the elongating forces just referred to.

As the cloud approaches the sun, the diameter is a greater fraction of its distance to the sun. Hence the difference in the force of gravitation, the elongating force, becomes greater. Furthermore, as the cloud is elongated, the differential force is further augmented by reason of this very elongation.

We can expect, then, that the cloud will be greatly elongated when it reaches, we will say, the region of the nearer planets, that is, when it becomes visible to inhabitants of the earth.

But there are other influences at work in the cloud as it approaches the sun. Let us examine a circular slice of the cloud, say at its center of mass, the slice being at right angles to the long axis. The particles in this slice will be sensibly at the same distance from the sun, and hence all subject to the same gravitational force toward the sun. All of these particles, excepting the outer ones in the circle, will be surrounded by gas of varying density. It is true that solid particles and denser fluids (gases) will fall more rapidly in an atmosphere than lighter fluids.

There will therefore be an action in the cloud similar to the action of a mixture consisting of a precipitate and non-miscible fluids of different specific gravities, though the action is not in all respects identical. The effect of the action just described will be to cause the solid and denser parts of the cloud to accumulate toward the forward part of the cloud. This concentration of the heavier parts of the cloud will shift the center of mass of the whole forward and still another action will take place.

As the center of mass is close to the superficial particles nearest to the sun, these particles will be subject to very much greater gravitation toward the center of mass than the superficial particles on the other side or end of the cloud. This differential gravitation within the cloud itself serves to condense the gases in "the head" of the cloud and to release the gas pressure and scatter the gases in the tail, further elongating the cloud. The falling gaseous cloud, it will be seen, in response to known laws governing the motion of bodies, has assumed the shape and arrangement of a comet and this by an ever intensifying process, just as we observe in the case of comets.

Before taking up further the analogy, suppose the original spherical cloud be given a momentum and direction of motion that would carry it in a circular orbit around the sun as a satellite having, say, a radius vector of five thousand million miles. Let us investigate the problem of the shape this fluid sphere will assume and the manner in which it will respond to the laws of motion and gravitation. Gravitation of the sun will act on this cloud, but it is not thereby drawn any nearer to the sun. The gravitation is exactly balanced by centrifugal force induced by the forced curved path of the cloud. That is, the centrifugal force on the entire cloud will be exactly equal to the gravitation. However, the law of the intensity of gravitation is not the same as that of the intensity of centrifugal force.

A particle on the side of the cloud nearest to the sun will have a greater gravitation toward the sun than a particle at the center of the cloud, in the inverse ratio of the square of the distance. The same particle will have less centrifugal force away from the sun, in the direct ratio of the distance. On a particle on the farthest side from the sun, the gravitation is less and the centrifugal force is more than on the central particle.

The resultant force on the near particle is thus toward the sun, and that on the far particle is away from the sun. This means that the cloud will be elongated in the direction of its radius vector, the elongation being limited by the gravitational force of the cloud itself.

Without other data than the mass of the sun, and of the cloud, the distance from the sun and the diameter of the cloud, a near approach to the elongation of the cloud can be reached by the solution of a differential equation. Without giving the derivation of this equation, the following is given as expressing the ratio between the half-diameters:

$$h/H = 1 + M/m\ (H^3/2D^3 + H^2/D^2 + H/(D + h) - H/D)$$

Where:

H = elongated half-diameter.
h = half-diameter normal to radius vector.
M = mass of sun.
m = mass of cloud.
D = distance to sun.

This is true, or approximately so, only when the cloud is at a great distance from the sun and is not excessively long. As an example: Let

$M/m = 100{,}000{,}000{,}000$ $D = 5{,}000{,}000{,}000$ miles. $H = 25{,}000$ miles.

Solution of the equation gives h = 7,100 miles. Thus at a distance of five billion miles a comet of the mass indicated is elongated in a ratio of 3.5 times its own diameter. This is the computed shape if the gaseous body were traveling in a circular orbit at the great distance of five billion miles from the sun. It would be invisible to us, however, for reasons to be stated later.

Traveling in an elliptical orbit the comet would be elongated in the same ratio at five billion miles, and as it approaches the sun, the elongation will be greatly increased, based on the same demonstration.

Let us pause here to say that a solid satellite is subject to similar elongating forces. Such bodies are elongated in the direction of their radii vectores either by internal stress, or else in their formative period they are permanently given an oval shape. The moon's excess diameter is 2.8 miles. Jupiter's first satellite has a computed ratio of diameters of more than two. Professor W. H. Pickering, with exceptionally clear conditions for observations, found Jupiter's satellites to be distinctly elliptical. (See *Ency. Brit.* 1907, Vol. IX, p. 274.)

This elongation of satellites which have no companion satellites is a full and complete explanation of the phenomenon, to which no exception is known, that all such satellites are turning one face toward their primaries at all times. They are forced to make one rotation in a revolution. Any tendency of an unattended satellite to rotate in the usual sense of the word would be stopped by the gravitational pull on the nearer protuberance or bulge as well as the extra centrifugal force on the far protuberance.

It has doubtless been anticipated by the reader that the action of the "cloud" as a satellite of the sun, traveling in an ellipse of great eccentricity, is a combination of that of the cloud falling into the sun, and the one traveling a circular orbit, and it partakes of all the elements of each, excepting the constancy of shape and internal equilibrium of the latter.

Mathematically, a falling body may be considered as a satellite traveling in an hyperbolic orbit of infinite eccentricity, which is a straight line.

The tail of a comet spreads out in the shape of a cone directly away from the sun. It is centrifugal force on this far-flung, rare, and distant part of the comet that compels it to assume this shape. When the comet nears its perihelion, or nearest position to the sun, the tail curves. The reason for this is easily demonstrated and easily understood. In negotiating the sharp curve around the sun, the outer parts of the tail have a much longer course to traverse than the head, but there is no force to accelerate their velocity sufficiently to enable them to keep pace with the head of the comet. Hence the outer parts of the tail lag behind and are lost to the comet proper. This gaseous matter continues, however, to travel in an elliptical course around the sun. At each perihelion passage, a comet loses a part of its mass in this way, and that matter continues to traverse an elliptical orbit near that of the comet. It is these scattered remains of a comet that afford us the meteoric showers that are seen when the earth passes through some defunct comet's path.

As a comet recedes from the sun, deceleration replaces acceleration, and it shortens, the tail being absorbed, so to speak. The comet shortens up as it recedes to its aphelion position, and its activity diminishes.

One of the hitherto unexplained phenomena of comets is the fact that comets increase greatly in brilliancy as they approach the sun and lose that brilliancy as they recede from it. The light is not reflected sunlight, except in a small measure.

My theory is that the light of a comet is due to electric activity. The rare gases, flowing out rapidly into the tail, by their friction on other layers of gas,

create electric charges and luminosity. The reverse process, as the gases flow back while the comet recedes from the sun, causes the luminosity in the comet as it recedes. When it passes out to a great distance from the sun, the activity greatly decreases, and the comet becomes dim or disappears. It is a matter of observation that the comet approaches an apparent spherical shape far out from the sun.

Split tails or split comets may be the result of electric charges of the same sign and repulsion between such charges.

I do not believe that any electric influence can pass between heavenly bodies. If such were the case, those bodies would be thrown out of their courses. Electric radiations, while they exert strong forces at close range, dissipate very rapidly. I see no reason to assume that a comet is in any way influenced by electricity coming, as such, from the sun.

Incidentally, I see no reason to believe that any electric or magnetic influence comes, as such, from the sun to the earth. The summer electric storm is due to the influence of the sun, but it is heat and light alone that come from the sun: the translation into electricity takes place locally in the earth's atmosphere.

If another incidental observation may be pardoned, I believe that the aurora borealis, the zodiacal light, the earth's magnetization, all have their origin in friction of high atmospheric layers as the upper air is forced in tremendously swift currents eastward at the equator and poleward, due to the mechanical influence of the moon and the convection due to the sun's heat in the torrid zone.

What is the origin of comets? It is clear that they are of comparatively recent origin. It is not at all probable that those which have closed or elliptical orbits come from outer space, and there are or have been many such comets. Some of them have worn out, so to speak.

We have only to consider the manifest mechanism of the sun to find all conditions necessary in the origin of a comet. The sun is manifestly a seething mass, spouting matter and gases into space at all times. A superficial band of the sun's fluid substance in the region of its equator is moving eastward more rapidly than the body of the sun itself. Some outside force must, of necessity, cause this motion. This outside force, as I believe, is Jupiter and the other planets, but this will not be discussed here. Suffice it to say that the motion of this equatorial matter must create great heat beneath the surface of the sun. What is more natural than that this sub-surface heat would gassify matter in the sun and that the pressure thus created would cause eruption of the overlying mass of the sun, a volcano, a sunspot?

If this matter and gas should leave the surface of the sun at a velocity of about 400 miles a second, it would travel out into space not to return, at least for many years. The gases being freed from the sun's gravitation and compressive forces, would expand into a great cloud. Out in space the cloud, under the influence of gravitation, would organize itself into a unit, as I have described at the outset.

But if a projectile were shot directly away from the sun, it would return, if at all, in the same direct course, neglecting the influence of the planets. However, if the projectile, or the gaseous cloud, should pass near one of the larger planets, it would be drawn in toward that planet and cross the plane of its orbit. This change in its course would cause it to take an elliptical course around the sun, and become one of the comets of our system.

In the nature of things, the sun would not fire a solid shot, rather the explosion, and in any event the fluid nature of the sun, would dissipate the matter; and the deep overlying gaseous layer, the atmosphere of the sun, would receive the full

force of the explosion, the momentum and velocity. Hence, instead of a solid projectile, we should look for a gaseous one, with some solid, broken up particles, exactly the constituents of a comet.

There are other possible origins of comets. Both Jupiter and Saturn seem to be hot and superficially fluid. The changing bands are evidence of this. Observations indicate in each planet the same thing in regard to these that is seen in the sun, namely more rapid rotation of the equator than toward the poles. This means the same motion of a band of the planet's matter near the equator, sub-surface heat and gassification, as well as volcanic eruption. The "red spot" of Jupiter, and the "bright spot" of Saturn, are probably volcanoes, corresponding to sunspots. Matter spouting from these need not be projected with such great velocity as sun projectiles in order not to return.

There are a number of comets that have their nodes near the orbit of Jupiter and others that similarly cross the ecliptic near the orbit of Saturn. This would be expected either if the comet had its origin in one of these planets, or if a stray cloud of matter, emitted from the sun, had been pulled into the ecliptic by the strong gravitation of either planet.

Comets are almost invariably discovered as they approach the sun and not as they recede, though failure to observe their approach may be taken as inability of the force of comet hunters to cover the entire heavens. A cloud being emitted from the sun would simply be a disorganized mass as it traveled out into space, and would scarcely be visible.

Sunspots are confined to belts near but not on the sun's equator. The sun's circulating equatorial belt would be thickest at the equator, and this accounts for the eruptions occurring where the belt is of less than maximum thickness. If these upheavals were on the equator, the earth might expect to be enveloped at any time in a blast of gaseous matter that would kill off all its inhabitants.

But because these gaseous clouds do not come from the sun's equator, and their velocity in the region of the earth's orbit is so great, that the earth's gravitation is scarcely sufficient to deflect their course very much, the inhabitants of the earth may feel safe in this regard.

Cometary orbits are very promiscuous. They do not follow the general rule of the planets of the solar system in traveling around the sun in direct motion, and their inclination to the ecliptic in some cases is very great, in contrast with the planets. It is just these characteristics of their orbits that would be expected if comets have their origin in the eruptions we call sunspots.

The fact is, if no comets had ever been discovered or observed, a study of the laws of matter as demonstrated and observed and a study of the action of the sun and the planets should lead us to look for gaseous satellites of the sun acting in just the way that the ordinary comet acts.

Meteors are probably very small particles of matter left in the trail of a comet at its perihelion passage. It is doubtful if any of them ever reach the surface of the earth. Friction in the earth's atmosphere furnishes the illumination.

Meteorites, on the other hand, the stones or pieces of iron and nickel that do reach the surface of the earth, seem to be of quite a different origin. Between the orbits of Mars and Jupiter are many hundreds, probably many thousands, of small bodies, each having normally an elliptical path around the sun. But when acted on by Mars or Jupiter, a change is forced in their path. It is quite possible that this change may force many of these small bodies into paths that would carry them near the earth. They would then be driven by the gravitation of the earth

into its atmosphere and to its surface. This is a perfectly logical explanation of the meteorite.

EDWARD GODFREY.

Professional Building, Pittsburgh, Pennsylvania.

General Notes

Dr. George Ellery Hale, Honorary Director of the Mount Wilson Observatory, known throughout the world as a leader and authority in all phases of astronomical research, died in Pasadena on February 22, 1938. A biography of Dr. Hale will be printed in one of the coming issues of POPULAR ASTRONOMY.

Dr. Francis G. Pease, member of the staff of the Mount Wilson Observatory for a number of years, an expert in designing instruments and using them in highly significant researches, died in Pasadena on February 7, 1938. A biography of Dr. Pease will appear in one of the coming issues of this magazine.

Dr. Edison Pettit, Astronomer at the Mt. Wilson Observatory of the Carnegie Institution of Washington, on March 16, 1938, gave, at the University of California at Los Angeles, a lecture on solar prominences illustrated by motion pictures which had been secured with the tower telescope of the McMath-Hulbert Observatory of the University of Michigan at Lake Angelus. Dr. Pettit was accompanied by Mr. Robert R. McMath, the Director of the McMath-Hulbert Observatory, who made a few informal remarks after the lecture. Dr. Pettit and Mr. McMath were introduced by Dr. Frederick C. Leonard, Chairman of the Department of Astronomy.

The Rittenhouse Astronomical Society of Philadelphia held a meeting on Friday, March 11, in the Hall of The Franklin Institute. The program was an address entitled "Precision Time-keeping" by Frank Hope-Jones, M.I.E.E., F.R.A.S., Managing Director of The Synchronome Company, Ltd., of London, England. On Thursday, March 24, Dr. Knut Lundmark, Professor of Astronomy at the University of Lund, Sweden, spoke on "Exploding Stars" before a joint meeting of the Rittenhouse Astronomical Society and The Franklin Institute.

Junior Astronomy News.—The issue of this publication for March has recently been distributed. As we have stated before in these pages, this magazine is published by the Junior Astronomy Club, whose headquarters are at the Hayden Planetarium, New York City. Each number consists of a dozen mimeographed pages which contain interesting, even fascinating reading. Recent discoveries are presented in a popular style, and well-established facts in astronomy are reviewed and oftentimes are given a novel and original flavor. One can read between the lines that the work in the preparation of the several issues is done enthusiastically and with pleasure. The organization is to be commended upon the success of this project.

George William Hill (1838-1914)

George William Hill, one of the greatest men of science this country has produced, was born one hundred years ago, on March 3, 1838. He was born in New York City in a house that has long since been torn down. He moved to Nyack

Turnpike (now West Nyack) as a young lad and there he spent most of the remaining years of his long life. He died in 1914 in a house that is still standing but is very much altered. He never married.

Hill's work in celestial mechanics places him in the very front rank of that difficult branch of astronomy, together with such other giants as Laplace, Lagrange, Le Verrier, Hansen, and Poincaré. His greatest contributions are the development of the notion of periodic orbits, the theories of Jupiter and Saturn, and new methods of approach to the theory of the moon's motion.

Two short but excellent biographies of Hill have been published. One of these is by Poincaré and serves as an introduction to his Collected Works published by the Carnegie Institution. The other is by E. W. Brown in the Biographical Memoirs of our National Academy of Sciences. A few years ago Dr. Frank Schlesinger placed a bronze tablet on the Hill Homestead at West Nyack commemorating the fact that Hill had lived in this house for many years.

Publications for Distribution.—We have a great excess over probable needs of the following Publications of the Flower Observatory: Vol. I, Parts 1 and 2 (3 out of print); Vol. II, Parts 1 and 3 (2 out of print); Vol. III complete, 3 parts; Vol. IV complete, 2 parts. The approximate total weight of these publications is 12 pounds. Vol. I, Part 1, describes the Observatory and instruments; the rest contain results with the Zenith telescopes and observations on double stars with the 18-inch refractor. As long as the supply lasts we will gladly give these away, in whole or in part, to amateur or professional astronomers, provided the person writing finds out the cost, by mail or express, of carriage to him and sends us this amount in advance.

CHARLES P. OLIVIER.

Flower Observatory, Upper Darby, Pennsylvania.

Centennial of the Loomis Observatory
May 7 and 8, 1938

The year 1838 marked the beginning of two small observatories which are at present the oldest in the United States. One of them, the Hopkins Observatory at Williams College, celebrated its centennial last autumn. The other, which was erected on the campus of Western Reserve College at Hudson, Ohio, was the first scientific institution of its kind west of the Allegheny Mountains. Both of these observatories are still in existence and in a fine state of preservation, although the observatory at Hudson is no longer a part of Western Reserve College. The College was removed to Cleveland in 1882 and has since become known as the Western Reserve University. At present an academy for boys, named for the older college, occupies the campus at Hudson.

In 1836, Elias Loomis was elected Professor of Mathematics and Astronomy at Western Reserve College and was commissioned to go to Europe to purchase books and equipment for an observatory. He returned in 1837, bringing with him a 4-inch equatorial, a 3-inch transit circle, and a clock. With the exception of the 4-inch objective, which is lost, these instruments are still in existence. The telescopes were mounted in September, 1838. Professor Loomis remained at Hudson until 1844, and during his stay made an accurate longitude determination of the place as well as establishing time service for the city of Cleveland, observed principally comets, besides making extensive observations of terrestrial magnetism and meteorology.

One of Loomis's successors was the eminent Charles A. Young, who served as Professor of Mathematics and Astronomy at Hudson from 1856 to 1866.

The Western Reserve Academy is planning to celebrate the centennial of the founding of this observatory on May 7 and 8 with a significant astronomical gathering and the following program:

MAY 7—2:00 P.M.

Colloquium—"Stellar Absorption Lines"

Speakers:

Professor Otto Struve of the Yerkes Observatory, "The Observation and Interpretation of Stellar Absorption Lines"; Professor Dean McLaughlin of the University of Michigan, "Peculiar Stellar Spectra"; Professor Donald Menzel of the Harvard College Observatory, "Theoretical Problems of Stellar Absorption Lines."

The members of the Neighborhood group of Ohio Astronomers and the Ohio Academy of Science are participating in this colloquium. All interested astronomers are cordially invited.

MAY 7—8:30 P.M.—Evening Lecture

Dr. Harlow Shapley of the Harvard College Observatory, "Sidereal Organizations." This lecture will be open to visiting scientists, the Cleveland Astronomical Society, and similar organizations as well as the public.

MAY 8—11:00 A.M.—Chapel Service

Professor Dayton C. Miller of the Case School of Applied Science, "The Service of Science in the World of Today."

Hudson, Ohio, is located 25 miles south of Cleveland. Astronomers interested in attending these meetings are requested to apply directly to Mr. R. H. Cleminshaw, Western Reserve Academy, for further information and accommodations.

J. J. NASSAU.

Warner and Swasey Observatory,
Case School of Applied Science, March 16, 1938.

Astronomy by Radio

In order to aid the layman to become better acquainted with the stars, WIP (Philadelphia) will begin a new series of programs on April 5, 1938, "My Stars," which will be broadcast each Tuesday evening from 8:00 to 8:45.

Conducting this series will be Mr. Armand Spitz, formerly editor and publisher of a suburban newspaper, and at present editor of the "Institute News," of the Franklin Institute. Spitz is a widely-known amateur astronomer, and lectures almost weekly on the popular aspects of astronomy.

This new series will present introductions weekly to some of the stars which may be seen in the heavens at that particular time. An unusual feature of the programs will be the suspension of the lecture for several minutes weekly after the evening skies are described, so that listeners may go out-of-doors and see the stars for themselves. During this unusual intermission, music (with the stars as themes) will be played, and the quarter-hour program will conclude with brief stories of the legends of the stars.

In addition, Mr. Spitz will send specially-prepared star charts of each week's talk in advance to any who request them. A complete set of these charts will make a valuable collection of simplified star guides.

Book Reviews

Instruments of Precision.—This is the title of an octavo pamphlet of sixty-four pages recently issued by the Gaertner Scientific Corporation, Chicago, Illinois. It contains numerous illustrations and detailed descriptions of scientific instruments of precision, such as Microscopes, Telescopes, Comparators, Cathetometers, Dividing Machines, Creep Test Microscopes, and others, which are made and distributed by this well-known firm. Indicative of the refinement of the instruments is a micrometer microscope which measures with an accuracy denoted by 0.00002 inch. The high standards obtained are the result of forty years of experience in meeting the progressively exacting demands of scientific investigation.

Comets, by Mary Proctor and A. C. D. Crommelin. (The Technical Press, Ltd., 5 Ave Maria Lane, Ludgate Hill, London. 8s 6d.)

A volume bearing the above title and the authorship indicated is sure to arrest the attention of the casual student of astronomy and also of the person well acquainted with the literature of this science. The former will respond to the allurement of the seemingly erratic objects, known as comets, which have frightened, mystified, and intrigued mankind from the earliest times to the present and which still await the final and conclusive explanation as to their origin and destiny. The latter will recognize in the authors two persons whose long and careful study of these objects entitles them to a hearing when they speak on this topic.

The present volume of two hundred pages is easily and quickly read. The reader is not required to follow lengthy and tedious computation or to decipher involved and abstruse theories. The writers have selected a few of the most spectacular of the comets and have given the high lights in their history to date. They have also given vivid and intimate stories of some of the most interesting personalities who devoted much time and energy to the discovery and the observation of comets. Among the comets we find those bearing the name of Pons (discovered at subsequent returns by other observers until now it bears the hyphened name Pons-Coggia-Winnecke-Forbes), Halley, Encke, and Biela; and among the observers, Hind, Tebbutt, Denning, Reid, and Barnard.

The final chapter is given to a restatement of "The Capture Theory of Comets" by R. S. Proctor written a half century ago, and a paper by Professor Crommelin "On the Origin and Nature of Comets," written a quarter of a century ago, supplemented by a few paragraphs to bring it up to date.

The book can be highly recommended to those who enjoy the simple and well-established facts of astronomy untrammeled by speculation and hypotheses.

Publications Received.—The publishers of POPULAR ASTRONOMY hereby acknowledge the receipt of the following named publications and express their great appreciation of the courtesy shown on the part of those who have sent them.

From the Dominion Astrophysical Observatory:

Vol. VI, No. 20. Spectroscopic Studies of Nova Lacertae 1936, by W. E. Harper, J. A. Pearce, C. S. Beals, R. M. Petrie, and Andrew McKellar.

Vol. VI, No. 21. The Spectrographic Orbits of H.D. 109510, by R. M. Petrie.

Vol. VI, No. 22. The Spectrographic Orbit of H.D. 214652, by Andrew McKellar.

POPULAR ASTRONOMY

CONTENTS

APRIL, 1938

ON THE VARIABLE MAGNITUDE OF GAMMA CASSIOPEIAE, Ernest Cherrington, Jr. 183

SHADOW BANDS, Richard L. Feldman 187

SOME METEOROLOGICAL OBSERVATIONS IN CONNECTION WITH THE 1932 AND 1934 TOTAL ECLIPSES OF THE SUN, Willi M. Cohn 201

NOTES FROM THE YERKES OBSERVATORY, Otto Struve 205

Planet Notes for May, 1938 207

Asteroid Notes 208

Occultation Predictions for May, 1938 210

Comet Notes 210

Comet 1935 *d* (Van Biesbroeck),—Comet 1925 II (Schwassmann-Wachmann),—Comet 1927 VI (Gale).

Variable Stars 211

Variable star notes from the American Association of Variable Star Observers,—A note on a Cepheid variable star.

Notes from Amateurs 214

Observations of Jupiter in 1937,—The New Haven Amateur Astronomical Society,—Cleveland Astronomical Society.

Meteors and Meteorites 216

Meteor notes from the American Meteor Society,—A peculiar hole near Tiffin, Iowa,—Chemical tests of "Oxide" from the Tiffin hole,—Contributions of the Society for Research on Meteorites: The great-circle distribution of the tektites.

Communications and Comments 231

Comets, their origin and behavior.

General Notes 236

Personal notices,—The Rittenhouse Astronomical Society of Philadelphia,—Junior Astronomy News,—George William Hill (1838-1914),—Publications for distribution,—Centennial of the Loomis Observatory, May 7 and 8, 1938,—Astronomy by radio.

Book Reviews 239

Instruments of precision,—Comets,—Publications received.

The principal articles of this magazine, beginning with Volume 15 (1907), are listed in the International Index To Periodicals.

MY

AE,
.... 183
.... 187
ON
UN,
.... 201
.... 205
.... 207
.... 208
... 210
... 210
ach-
... 211
Ob-
... 214
mi-
... 216
ear
ibu-
stri-
... 231
... 236
hia,
ica-
and
... 239
), are